G. III.

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A CONTINUATION OF the NEW DIGESTER OF BONES:

It's IMPROVEMENTS and new USES it hath been applyed to, both for *SEA* and *LAND*.

Together with some Improvements and new Uses of the *AIR-PUMP*, tryed both in *England* and in *Italy*.

By *DENYS PAPIN* M. D. Fellow of the *ROYAL SOCIETY*.

LONDON,

Printed by *Joseph Streater*, near *Pauls-Wharf* in *Thames-street*, and are to be Sold by the *Book-Sellers* in *London*. 1687.

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To the Right HONORABLE

JOHN

EARL OF *CARBERY*,

PRESIDENT

OF THE

ROYAL SOCIETY.

My Lord,

IN this small Treatise may be seen good Improvements of two useful Engines, together with several Uses the said Engines have lately been apply'd to: Your Lordship knows that most of them have been try'd in the *Royal Society*, and they could not fail to produce some good Effect, since in making of them I was conducted by Men of that Capacity; so that I am ready at present to convince by Experience all those that would question either the Truth, or the Usefulness of the Contents of this Book: It may therefore be of great Advantage to the Publick, if I can but keep it from lying in the Dark, and engage People

to take Notice of it: Now My Lord, for that purpose I know no better way than to Inscribe it to Your Lordship, who possessing so many great Qualities not to be described in this place, is also the head of that *Illustrious Body, The* ROYAL SOCIETY, which is deseruedly so highly esteemed wherever Learning is valu'd: I hope therefore, that this Dedication which might at first be taken for a great Boldness, will plainly appear to be but an indispensible Duty since the leisure I have enjoy'd to follow these Studies is owing to that learned Company, where Your Lordship doth Preside with so much Aplause, and these your many Benefits are bestow'd upon me to no other purpose, but to engage me to contribute whatsoever I can that Your Generosity may turn to the Advantage of the Publick. I am with all Imaginable Respect,

My LORD,

Your Lordships

Most Humble, and most Obedient Servant.

D. Papin

TO

TO THE READER.

THis Treatiſe of the Digeſter, as well as the former, preſents me with an occaſion to ſpeak againſt an Error, that may do great prejudice to the Publick: The thing is that moſt of the Inventors do think it neceſſary to keep their Diſcoveries very ſecret, for fear other People imitating them ſhould have a ſhare in their profits; this falſe Opinion hath bereau'd the World of ſeveral good ſecrets, and the Inventors have alſo been deprived of all their poſit, becauſe they were afraid to looſe part of it. Now we may eaſily avoid falling into that Error, if we do but conſider, that People are not ſo forward to embrace new things, every one looks to himſelf, and is glad to ſee others try firſt the way: Of this truth we may find a good Inſtance in the following Papers, ſince they ſhew plainly enough that the Digeſter is an uſeful Invention, grounded upon good Principles, and confirmed by Experience: Nevertheleſs having publiſht this Diſcovery above five Years ago, I ſee that but very few People have been willing

ling to make use of it: It is known likewise that when the Invention of Wind-Mills *and* Water-Mills *was new* Pliny, *although he was one of the most knowing Men in that time, did not value those Engines but as a Curiosity; and their usefulness could not be well approved, but by length of time: I do therefore conclude, that those who make new Discoveries must take care to make them perfectly known to the Publick as soon as they can, that the World being fully convinced of their usefulness, they may be brought to ordinary practice, and this will still happen, but too late for the advantage both of the Publick and of the Inventor: For my part that I may not be found wanting in promoting the Engines treated of in these Papers, I do not only explain as clearly as I can, all that I know about the same; but I undertake to let People see them try'd once a Week, in* Black-Fryars, *in* Water-Lane, *at Mr.* Boissonets, *over against the* Blew Boot; *every* Moonday *at three of the Clock in the Afternoon: but to avoid Confusion and crouding in of unknown People, those that will do me the Honour to come, are desired to bring along with them a Recommendation from any of the Members of the* Royal Society.

SECTI-

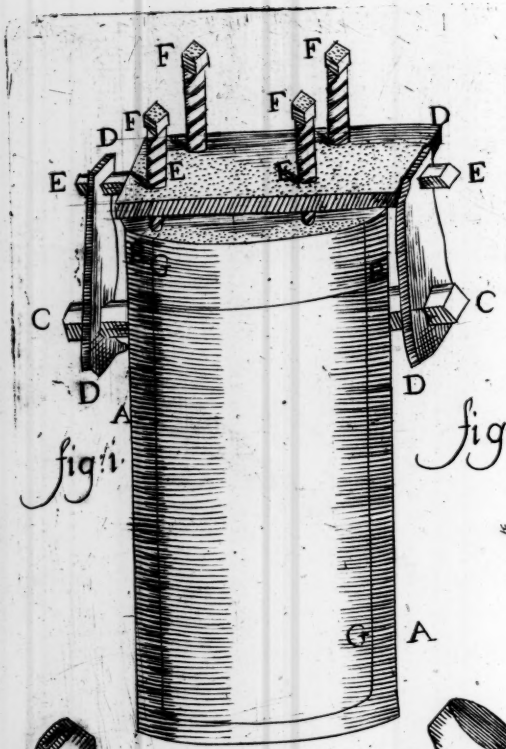


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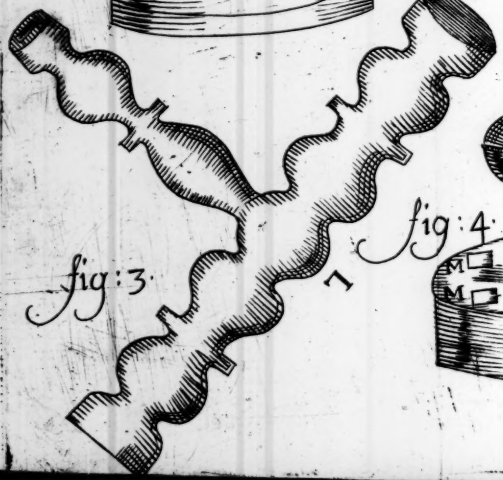


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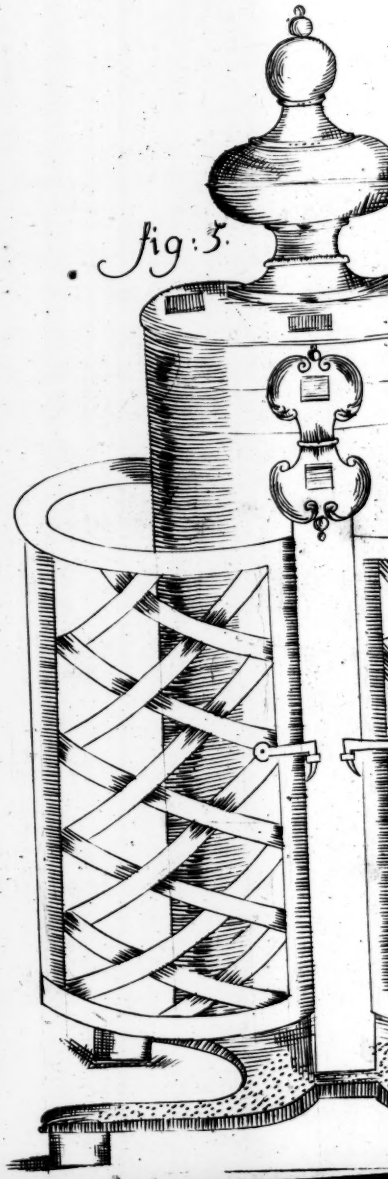


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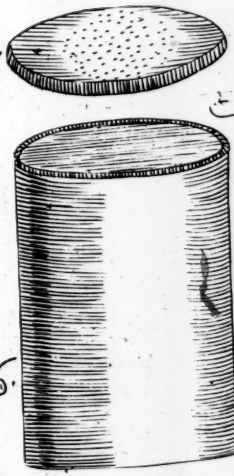


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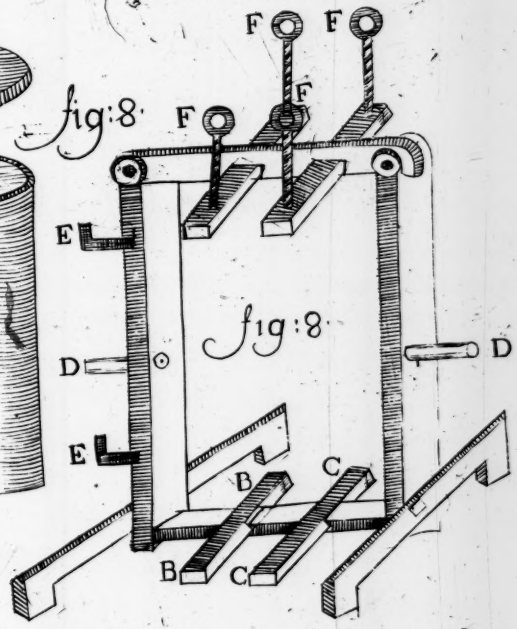


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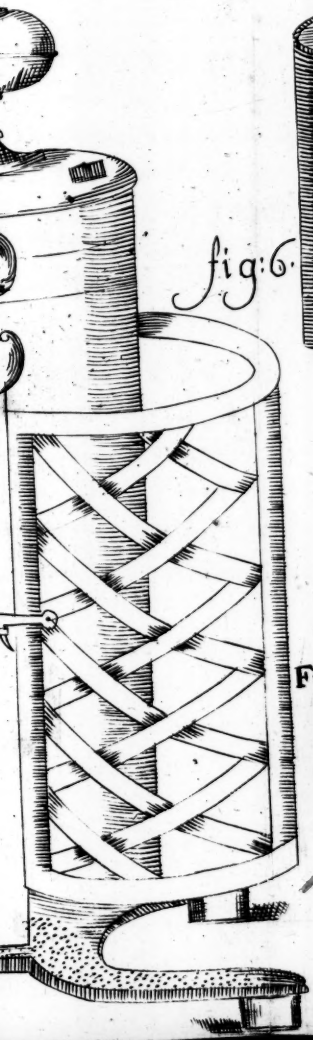


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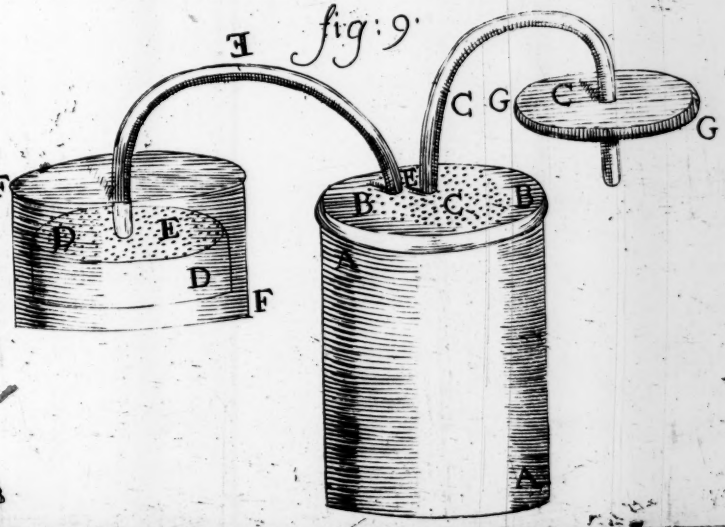




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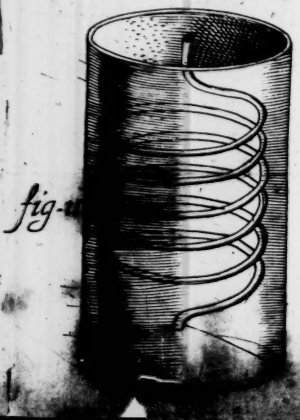


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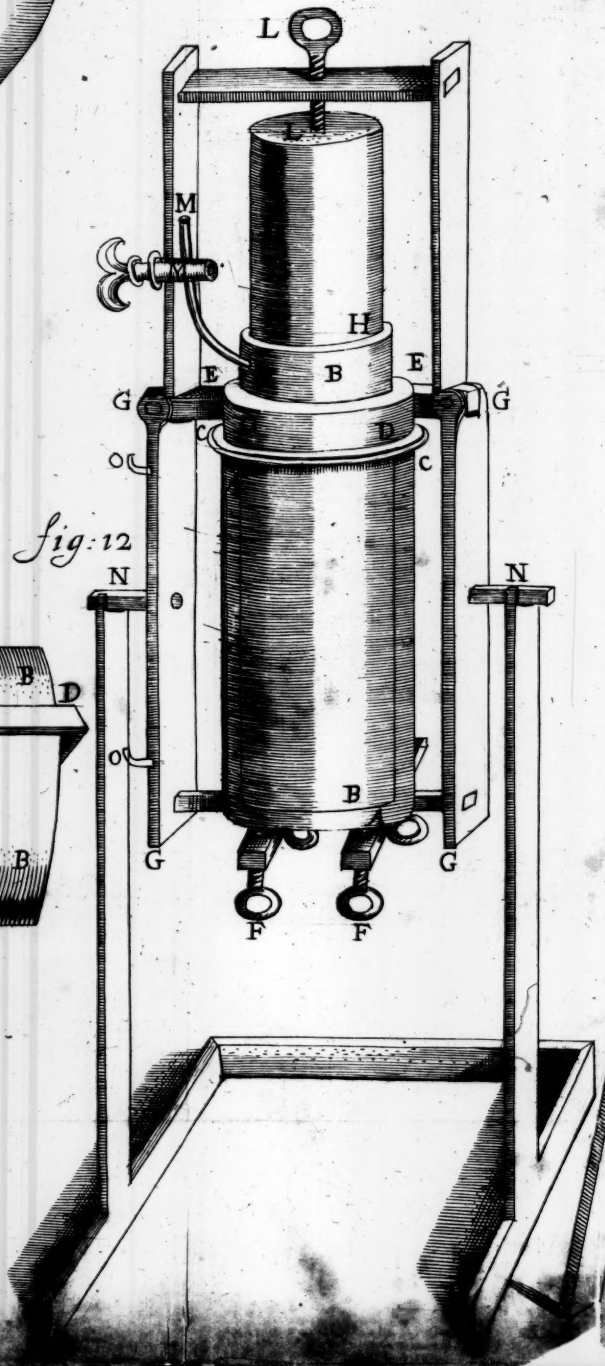
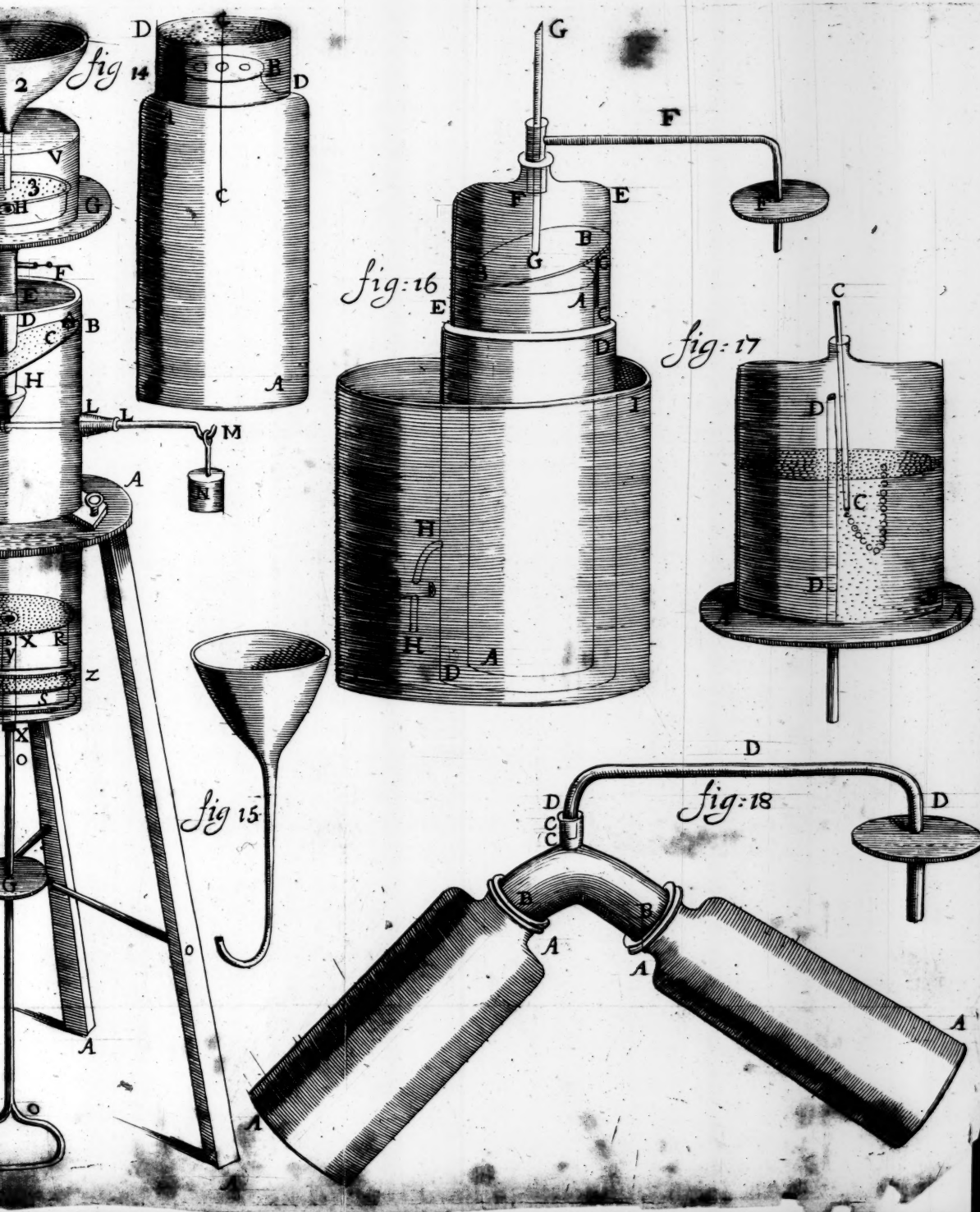


fig. 12.



fig. 13.


fig 14
fig: 16
fig: 17
fig 15
fig: 18

SECTION

The FIRST.

Of the IMPROVEMENTS *and new* USES *of the Digester of* BONES.

THE first Improvement I have made of the Digester, was when His Majesty *CHARLES* the Second of Blessed Memory, commanded me to make One for His *Laboratory* at *Whitehall*; I made it such as it is represented by *Fig.* 1st.

A A is the great hollow Cylinder of cast *Brass*, 12 Inches high, and 5 Inches Diameter.

B B is the Cover of the Cylinder, 2 Inches deep, and is ground and adapted very exactly to the Cylinder.

C C two Appendices of the Cylinder *A A*, as the Tronions of a Great Gun.

D D D D two pieces or Plates of Iron, having holes to their ends, to enter by one end upon the Tronions *C C*, and by the other upon the Iron Plate *E E E E*.

F F F F Four Screws who have their female in the Iron Plate *E E E E*; So that by screwing of them, the cover *B B B* is prest against the Cylinder *A A*.

G G is the inward Pot to receive what is to be digested, it is held up by means of three little Feet, at some distance from the bottom of the Cylinder *A A*; and the aperture reacheth as high as possible in the Cover *B B*, to the end

 the

the ſaid aperture, may ſtand above the Water contained in the Cylinder A A.

To ſhut up the Pot G G, a cover which needs not be very exact is put upon it, a linen cloth is put upon that cover, and a *Braſs* Ring I I. *Fig.* 2. Is thruſt over all; ſome Water may be powred into that Ring before it be quite thruſt in: The Pot thus ordered is to be put in the great Cylinder A A, which then muſt be filled up with Water as much as is poſſible, and then apply upon it its cover B B, which thruſteth down the Ring I I; the water which had been put in it remains above the commiſſure: So that the paper Ring which is to be put betwixt the Joints remaineth wett ſtill, which is neceſſary to ſhut this Engine exactly, as I have ſaid in the firſt Treatiſe of the Digeſter, *Chap.* 1. The Cover B B being ſo applyed, the *Iron* Plate E E E E is to be adapted to the pieces D D D D, as it is repreſented in the Scheam; then by turning the Screws with the Key L L. *Fig.* 3. which hath a ſourſquare hole to enter upon the ſquare heads of the Screws; the Cover B B is preſt very hard againſt the Cylinder A A. Now that the *Iron* Plate E E E E may be always ready upon the Cover B B, there is a piece of caſt Braſs turned; ſuch as is repreſented *Fig.* 4, made firm upon it; this piece is piereed with four holes M M M M through which the Key L L may be put to turn the Screws.

N Is another hole through which drops of Water may be put upon the Cover B B, to ſee when it is enough. *

O O A certain Cartouche born by a movable Ring, which by being a little moved round, to and fro, ſhuts or opens the holes all at once; but that is only for Ornaments ſake, and that the Engine may appear very plain; and indeed being

* *The way to know the Degree of Heat by means of a drop of Water, hath been explained in the firſt Treatiſe of the* Digeſter, *Chap.* 1ſt.

ſet

ſet up upon its Furnace, *vid. Fig.* 5, it ſeems to be but an ordinary ſingle Pot with a Cover, and a Furnace a little embeliſhed.

This Engine hath the advantage to have no hole to the cover, which in the firſt Engines cauſed ſome trouble beſides the danger, leaſt ſuch a hole ſhould open and ſpoille the Operation; It is alſo a great conveniency that the inward pot hath it's aperture conſiderably above the water it lies in: for this will prevent the danger of the waters getting into the pot, and ſpoiling the things that are a boiling, ſome comunication may then be left to the Air from one veſſel into the other, and ſo, tho' the inward veſſel be very weak it will not be in danger of breaking, whereas I have obſerved in the firſt Treatiſe of the Digeſter, that it hath happened ſometimes, that the liquor in the veſſell A A, hearing before the inward pot; made a preſſure ſtrong enough to break the inward pot, tho it were pretty ſtrong: I do therefore conclude this new Way will coſt leſs, and will be more eaſie to be uſed than the former; And I do not ſee any thing better can be done, for things that are to be made ready in their proper Juices, and without Water.

Nevertheleſs, as there is abundance of things, that are to be boil'd with Water, as Gellies, Beans, Rice, &c. I have made for that deſign another Engine, which will be very advantagious, as well for the cheapneſs of the Engine, and the ſaving of fire and time, as for the conveniency of uſing of it; this Engine is nothing elſe but a hollow Cylinder, ſhut at the bottom, and open at the top, repreſented *Fig.* 6. I line it within with Pewter pretty thick, for fear the Braſs ſhould give ſome bad quality to the Gelly: And ſo there is no need of an inner Pot: We muſt fill up the ſaid Cylinder (*fig.* 6.) with Water and Bones, and leave about an Inch empty, more or leſs, according as the Cylinder will be more or leſs than 12 Inches deep. The cover repreſented *fig.* 7. muſt alſo be lined with Pewter, and may be quite flat; becauſe this Engine being fitted to be turned upside-down

when we pleaſe; the Paper betwixt the Cylinder and the Cover may be preſerved always wet, and ſhut exactly, and there is no need the Cover ſhould have any depth, as there was in the other Engines. This Digeſter is to be ſet in an Iron Frame; repreſented *fig.* 8. ſuch a Frame is very fit to preſs very hard; becauſe the pieces of Iron C C. B B are a little higher at their extremities than in the middle, and they are ſo placed, that the Cylinder bears upon them in four places, directly oppoſite to the four Screws F F F F, which preſs at equal diſtances upon the Cover, ſo that the preſſure being ſo equally diſtributed on all ſides, it ſhuts much ſtronger, than when there is but one ſide which bears the greateſt part of the ſtrength. The ſame Iron Frame hath two great Iron Pins D D, upon which it may eaſily be turned up-ſide down, being ſuſpended by the ſaid Pins, either by Ropes, or upon ſome Iron Foot. There are alſo two Hinges of Iron E E, fit to receive an Iron Furnace, which holds the Fire to the Engine, and in the time it is heating, it ought to be ſet *Horizontally*, that the Fire being directly underneath, may heat it the ſooner.

As meat is apt in ordinary Pots to ſtick to the ſide of the Pot that toucheth the Fire, it might alſo be apt to engage it ſelf in this Digeſter; ſo that tho' it were turned up-ſide-down, the meat might ſtill remain in the ſame place, and ſo that part towards the Fire would be in danger of burning: I have thought of a way, how I may keep always Water between the Pot and the Meat, on the ſide of the fire: I take for that intent a plate of Pewter, bent like a portion of a hollow Cylinder, having ſome ſmall Feet to bear it: I put this pewter Plate in the Digeſter, on the ſide the fire is to be, and I put the Meat upon it: And ſo I am certain there will always be the height of the little Feet of the Plate fill'd with Water, betwixt the fire and the Meat, to preſerve it from *Empireum*: This pewter Plate is neceſſary only for Meat; for I have not found that there is any danger of burning Bones, Harts-horn, or Ivory, in making of Gellies.

It

It is easy to see that for the Boiling of things which require Water: This Engine hath great advantages above those which have two Pots: For 1*ſt*. The inward Pot being here as it were incorporated with the outward Cylinder, it Fortifieth it, and requireth not so much Metal, as when there are two Vessels. 2*dly*. More Gelly may be done in this Digester at once than in the others; For the inward Pot can never hold so much as the outward. 3*dly*. There is less Coal spent in the heating of it: Because the Fire being directly under the Engine, it acts more powerfully than when it is put only by the side, and by the same reason the Operation being quicker finished, it can be repeated more times in a day. 4*thly*. Having the conveniency to move and shake this Engine, there is reason to believe, it will by that means make a much greater effect, in the Extracting of Gellies: Because the Gelly is nothing else but a dissolution of Glutinous Particles in Water, and it is known that Agitation is of a great help to hasten Dissolution. 5*thly*. This Digester may be open'd a great deal hotter and sooner than the others: Because if one opens a Digester with double Pot, before it were almost cold; it would happen that the heat and pressure, being yet pretty great in the Internal Pot, the Liquor in it would Boil high and run over, as soon as it would be freed from the Ambient Pressure, and the Liquor thus spil'd is spoiled, by being mingled with the Water of the External Vessel: But with this new Engine we may take out the Gelly as soon as it is cooled enough, to evaporate the drop of Water in 25 or 30 seconds: Because the screws may be loosen'd by little and little, till the Liquor falls like a thred, and the Engine being set so that the Liquor may fall straight downwards, without touching any thing that might spoil it, a Vessel may be put underneath to receive it: Much time may by this means be saved, and the Operation may be reiterated many more times in one day, than it could be done with the other Engines. 6*thly*. It is Plain that this Engine may serve at Sea even in such storms, where the

the Liquors in ordinary Kettles would be ſpilt by the ſtrength of the Agitation: For this Engine being faſtened to ſomething in the Ship, there would be no danger it ſhould looſe any thing; and the Furnace alſo might be ſhut in ſuch a manner, as would prevent all the accidents of Fire. *7thly.* and Laſtly, there being but one pot here to be ordered, there is no doubt but this new Digeſter is more eaſie to be made uſe of, than thoſe that have two Pots.

Care is to be taken, to make the lining of Pewter of ſome thickneſs in this Engine: Becauſe if it was only tin'd over, this tinning would be quickly worn away, and the Braſs would give a bad taſt to the things made ready in it: I have Experimented it, and have wondred to ſee how quickly the tining becomes defective: Nevertheleſs, when there is ſome thickneſs of Pewter, theſe Engines will laſt long enough, and I have not perceived that pewter Pots uſed for ſeveral Years in the Digeſter were ſenſibly diminiſhed: But it muſt be, that in ſome places the tining is thin beyond imagination.

Experiments tryed with this new Digeſter.

EXPERIMENT *1ſt.*

TO know more preciſely than by meer conjectures, how much this new Digeſter would ſave in Fire: I took one day two parcels of Char coal, of a pound and half each; and being both of them kindled equally, they were put at the ſame time the one to the Digeſter repreſented *fig.* 5., and the other to the Digeſter repreſented *fig.* 6. 7. 8. the Fire was encreaſed equally to the one, and to the other: there was in the firſt but what was neceſſary to make four pounds an half of Gelly, and in the other there was enough to make ſix pounds, nevertheleſs the firſt came not ſo ſoon

as the other to the heat neceſſary to make Gelly of Ivory: So that, what remained of Coals, to the one and to the other being weighed, more than twelve ounces were found to have been conſum'd to the firſt; whereas to the other there had not been nine ounces conſumed: So that it appeared by this Expereriment, that by the new faſhion Digeſter there was almoſt half the Fire ſaved: Since it did make a quarter more Gelly, with a fourth part leſs Coals then the other.

EXPERIMENT 2d.

Having ſeen by the preceeding Experiment, that this new Digeſter would ſave Fire much more than the old Ones, even for thoſe which ſhould have occaſion to make uſe of it but now and then: I thought that thoſe which would make a conſtant uſe of it, either for ſome trade, or for nouriſhing the Men of a Ship, could alſo find in it much greater Advantages; this hope was grounded upon the conveniency one hath to open this *Engine* before it is cold; for being filled up with new Materials as ſoon as it hath been emptyed, being pretty hot ſtill, I doubted not but it could be heated ſooner, and with leſs *Coals* than the firſt time; But to know more exactly what that might amount to, I did once make a Tryal of it.

I encreaſed the Fire to the new Digeſter, till it was come to the ordinary heat to make ſix pounds of Gelly, and being left about a quarter of an Hour to cool, I took out the Gelly and filled the Engine again immediately with new Materials: I did put afterwards the Fire to it with equal weight of Coals, and I found, that whereas the firſt time it could not be heated in leſs then 36 Minutes; this ſecond time it required no more but 27, and there was but ſeaven Ounces and a half of Charcoal conſum'd; and ſo I conclude, that by this

this new manner, there would be a sixth part of Coal less spent, than I had found by the preceeding Experiment; there is no doubt also, but the Engine would compleat its Operation, and make six Pounds of Gelly in less then an hours time; and therefore it is easy to see it can afford about 150 Pounds in four and twenty hours, by consuming but about eleven pounds of Charcoal, and if the Furnace is made fit to burn Sea Coal, the charges will be yet much diminished. It may therefore be judged how much such an *E*ngine will be useful in long voyages at Sea, where people are tired and sick with eating salt meat.

EXPERIMENT 3*d*.

I would afterwards try if the conveniency to move and shake this new Digester, would not afford the advantage of making Gelly with a lesser degree of heat: for that purpose I took the same proportions of Water and Ivory as in the preceding *E*xperiments, and having filled up the Digester *fig*. 5. with the quantity necessary to make four pound and half of Gelly, and the Digester *fig*.6. 7. 8. with the quantity necessary to make six pounds: I heated them both till the first evaporated the drop of Water in six seconds, and the other but in twelve: Nevertheless, the Gelly was found as firm in this last as in the first; and so I doubted not but the shaking had a great effect to hasten the *E*xtraction of Gelly, as well as it hath for all other Dissolutions.

EXPERIMENT 4*th*.

In these first Experiments the Furnace was made of an Iron Plate, bent into a half Cylinder; which did very well hold the

the Fire to the Engine when it was laid horizontally, but it hindred the Aire from coming underneath to kindle the Fire, and carry the heat upwards to the Engine, I believed then there would be ſome advantage, to make the under part of this Furnace like a Grate; or pierce the Iron Plate with a great quantity of holes; it ſucceeded prety well: For inſtead of 36 minuts that had been required the firſt time, to heat the Digeſter: when the Furnace was full of holes, there was no need of more than 25 minuts, and there was no need almoſt of blowing.

EXPERIMENT 5th.

I would alſo try what quantity of Gelly would be made with a certain qnantity of Ivory: I took therefore a pound of Ivory, which I ſhut up with 6 pounds of Water in the new Digeſter: I encreaſed the Fire till the drop of Water did evaporate in twelve ſeconds, and having left the Engine cool enough to be opened commodiouſly, I took out the Liquor, and ſqueezed the Ivory thro' a linen Cloth as much as I could: So that I had at leaſt 5 pounds of Liquor, which did Congeal that night: I did put again the ſame Ivory in the ſame Digeſter, with the ſame quantity of Water, and having encreaſed the Fire till the drop of Water did evaporate in 9 or 10 ſeconds, I took out the Liquor, and ſqueezed the Ivory thro' a linen Cloath as before: So that I had again more than 5 pounds of Liquor which congealed alſo during the night: I put again the ſame Ivory in the ſame Digeſter with the ſame quantity of Water, and I encreaſed the Fire this time till the drop of Water would evaporate in 4 ſeconds: I did afterwards take the Liquor and ſqueezed the Ivory in a linen Cloth as before; and I got again at leaſt five pounds of decoction, which congealed as the others, altho' it

was then in the month of *May*, and it was prety hot for that time of the year: I conclude therefore from this Experiment, that one pound of Ivory ſhavings, will afford at leaſt 15 pounds of Gelly: I might even have drawn ſome more Gelly from the ſame Ivory, but it appeared to me ſo much ſoftened, that I beleived that which remained was not worth the drawing, and I was further confirmed in that Opinion by the following Experiment.

EXPERIMENT *6th.*

I would once try how Troters Bones would ſucceed if I extracted Gelly from them ſeveral times conſecutively, as I had done with the Ivory in the preceding Experiment: I did therefore put Troters Bones with Water in the Digeſter. But there was but 4 pounds of Water upon two pounds of Bones: I repeated the Operation four times upon the ſame Bones and I increaſed always the Fire to evaporate the drop of Water in four or five ſeconds: I broak alſo every time the Bones in ſmaler peices, that the new Water might ſo much the better penetrate them; it happened that all the four times I had Gelly about four pounds every time; but the fourth it was ſo weak, and the Bones were ſo brittle, that I judged it would be uſeleſs to boil them any more: Now as Ivory ſhavings are much eaſier to be penetrated than thick Bones, I doubted not but the three coctions of the Ivory in the preceding Experiment, were as much as it would bear: So that I believe that when fifteen pounds of Gelly ſhall have been extracted from one pound of Ivory ſhavings, we muſt be contented: It may be that Bones alſo would give no leſs, if they were well pounded: But till we have ſome good Inſtrument to grind them eaſily, and in quantity, I believe it would not be worth while.

Ex-

EXPERIMEMT 7th.

Becauſe it is likely that this Engine will henceforth be a neceſſary Inſtrument at Sea, where there is with the meat a great quantity of ſalted Bones, which are generally thrown away, and out of which good freſh Gelly might be drawn, which would be a food much wholſomer than the Meat it ſelf: I would make my ſelf ſure of it by Experiment: I did therefore once put in a great earthen Pan, a good quantity of Ox Bones, which I covered over with Salt; and after I had kept them ſo the ſpace of a fortnight, ſo that they ought to be as much ſalted as Bones can be, I did put them to ſoak in Sea Water, as meat is upon Ships, and being afterwards put in the new Digeſter with double their weight of freſh Water, I encreaſed the Fire till the drop of Water did evaporate in four ſeconds, and I found a ſtrong Gelly, good and freſh: I reiterated afterwards the Operation, with the ſame Bones and new Water, and I had very good Gelly again, as well as if the Bones had never been ſalted: So that there is no doubt, but by this Engine a Noriſhment may be had upon Ships whoſe materials will coſt nothing, and which will nevertheleſs be better and wholeſomer than the Meat which is dear, and this matter will even cauſe no encombrance, ſince it is however carried; for in ſalting the Meat the Bones are left with it, tho' they be then thought of no uſe.

EXPERIMENT. 8th.

To confirme yet more the uſefullneſs of this Engine in Voyages at Sea, Sir *John Hotham* thought fit to make an Experiment upon *Rice*, being well informed that it is a

wholeſome Food, which doth agreat deale of good, and which would be much uſed in long Sea-Voyages, were it not for the great quantity of freſh Water and of Fire which is conſumed to make it ready : I did then one day ſhut up a pound of *Rice* with three pounds of water in a Digeſter of the new faſhion, and encreaſed the fire but to evaporate the drop of water in twelue Seconds, nevertheleſs I found that the *Rice* had ſuckt up all the Water and was extreamly ſweld, and the grains had almoſt all kept their ſhape yet they were pretty ſoft: ſo that I doubted not that if I did put a greater Proportion of Water with the *Rice* I ſhould make it as ſoft as may be, I did then ſhut up half apound of *Rice* with two pounds and ahalf of Water, and I did put alſo above half an ounce of ſalt to ſee if ſome quantity of Sea Water might not be mingled with the freſh when *Rice* ſhall be made ready at Sea: I encreaſed the Fire till the drop of Water did evaporate in eight ſeconds, and I found that the Rice had yet imbibed all the Water, and ſo was much more ſwel'd and ſoft than the firſt time, yet the grains had ſtill kept their ſhape, but they might eaſily be reduced into pop with a Spoon, ſo that it would be uſeleſs to ſoften them more; it was alſo well ſeaſoned neither too freſh nor too ſalt: So that it was plain, that not only the Digeſter can remedy the Inconveniencies that are found to make Rice ready at Sea: But that even inſtead of conſuming freſh water, it will make a good quantity of Sea Water ſerve as uſefully to nouriſh as if it had been freſh Water. I had already made an Obſervation almoſt like this, upon Peas and other Pulſe, but as it hath been publiſh't in the firſt Treatiſe of the Digeſter *Chap.* 3d. *Exp.* 10*th.* I ſhallnot repeat it here : *I* ſhall but take notice that according to the Experiment that Mr. *Boyl* hath made to find the proportion of ſalt which is in Sea-Water: in making *Rice* ready about fowr parts of Sea Water ought to be put upon three of freſh : and by that means it would be ſalted about as much as that of this Experiment was.

Ex

EXPERIMENT. 9th.

I have had alſo the Curioſity to try if the new Digeſter would not be uſefull to Brew Beer, having the conveniency to be Heated in a little time, and the great Motion that can eaſily be given it, being fit to contribut much to the extracting of the Tincture of the Malt: I did therefore ſhut up a pound and a half of Malt with about ſix pounds of Water, and I encreaſed the Fire till the drop of Water did evaporate in ſix ſeconds; the tincture was very ſtrong and of a good taſt; but I believe the exceſs of heat had deprived it of it's Fermentative Virtue: For having mingled it with a pretty good quantity of yeaſt, it did not ferment nothing nigh ſo much as Beer uſes to do: I did therefore repeat the Experiment this ſecond time' I encreaſed the Fire but to make the drop of Water evaporate in ſixty ſeconds, and I ſaw that then I had a tincture which did ferment very well, and became tranſparent: I troubled not my ſelf to try preciſely what quantity of Coals would be conſumed; becauſe for this Operation, much greater Engines ſhould be uſed and the expence of Fire would be leſſer with big Engines, than with little ones: So that I ſhall only ſay, that the Extraction of the tincture of Malt requiring not much heat, the Operation might be repeated many times a day, and ſo a great quantity of Beer may be made in a ſmall time, and with but little Fire.

EXPERIMENT 10th.

I have alſo tryed the effect of the new Digeſter, for the Extracting the Tincture or Juice of Liquoriſh; becauſe being a hard root, and which can't be well penetrated, but by a ſtrong coction. I thought it likely the Digeſter would be

be uſeful in this occaſion: I filled up then a Digeſter with Liquoriſh roots and Water, and having encreaſed the Fire till the drop of Water would evaporate in two ſeconds, *I* found the Liquor in the Pot had acquired a dark red Colour, and that in five or ſix hours time it coagulated, becauſe of the great quantity of Juice that had been drawn from the Liquoriſh, by that ſtrong Operation. *I* mingled afterwards part of this Juice with ſome Gelly of Bones and a little Sugar, and having ſet it to dry, it became Clear and Tranſparent, and fit to be carried in the pocket, without danger of melting or puttifying; being melted in the mouth it had the taſt of Liquoriſh, much ſtronger than any Juices of Liquoriſh, which are ſold very dear in *London*.

EXPERIMENT 11*th*.

I have alſo tryed *Chocalat* in the new *D*igeſter becauſe it is a matter which looſes much of it's moſt volatile Spirits; when it is prepared after the ordinary way, it muſt be wrought upon three ſeveral times before it is ready to be drank, and every time takes away part of its uertue: *I* believed then *I* might remedy theſe inconveniencies by means of this Engine, which lets nothing to evaporate away, and having put four ounces of Chacolate Nuts with a little Cinamon, and about two pounds of Water, in a Digeſter of the new faſhion, *I* gave it almoſt as much heat as would be needful to ſoften Bones, *I* found a tincture of Chacolat very ſtrong, and the fruit extreamly ſwel'd and ſoft, but yet it conſerved it's ſhape, *I* took away the Liquor and put new Water upon the ſame fruit, and having given it again the ſame degree of heat as it had the firſt time, *I* had again a pretty ſtrong tincture of Chacolat, but weaker than the firſt: *I* took away this Liquor, and put again new Water upon the ſame fruit, and having given the ſame degree of heat, *I* had a-

again a tincture of Chacolate; but much weaker than any of the firſt; the fruit was ſo ſoft, that it might eaſily be reduced into dow betwixt the fingers; but yet it had ſtill kept it's ſhape, tho' it had almoſt loſt all it's taſt: *It* is likely then, that without looſing any thing, all that is good in the Chacolat may be extracted from the Glutinous and Terreſtrious parts; which parts Sr. *Edmond King* hath told me have very bad effects: Notwithſtanding, if people would rather chooſe, to take all the ſubſtance of this Fruit, it would be eaſie when it is ſoft, to bruiſe it in a wooden or marble Mortar, and ſo mingle it with the Tinctures, that had been extracted from it: by that means, beſides what is loſt by the evaporation, in grinding it after the ordinary way, the greateſt part of the Work, and of the time that muſt be ſpent upon it, would be ſaved alſo.

EXPERIMENT. 12.

I have alſo tryed to ſoften the hardeſt Woods ſo as to Print upon them Carveing work as they do at *Nurcmberg* upon ſome tender Woods with which they adorn boxes which are ſold in quantity through all *Europe.* I put one day ſome peices of *Lignum Vitæ* with ſome *Therebentine* in the Digeſter, and I gave it agreater heat and longer time than had been needfull to ſoften bones: But it was in vaine: for the wood did not at all appear ſoftned, I would afterwards try if the Spirit of *Turpentine* as being more penetrating would not ſucceed better, but I had the ſame ſucceſs again: I did at laſt make the Experiment with Water, and tho' I had not beleived it fit to act upon a Reſinous wood as *Lignum Vitæ* is, yet it ſofned it enough to receive eaſily the imprint from a peice of Coin, but it muſt have time to dry under the peice that gives the impreſion; otherwiſe the wood will return by its ſpring and the print is thereby in part de-

defaced; but when it is well dryed by the Fire at the ſame time it receiveth the impreſion, it preſerves it very well and even remaineth harder then it was before it had been ſoftened, I have made the ſame Experiment with *Box*, and have found it yet fitter then *Lignum Vitæ* for theſe ſorts of works, ſo that *I* beleive that by means of the Digeſter it wovld be eaſie to make ſomething better than what is done at *Nuremberg*.

EXPERIMENT. 13*th*.

I would alſo try what could be done with Cows Horns: *I* took then one day ſome peices of Cows Horns which having been boilled in the Digeſter and afterwards dryed were become brittle, ſo that *I* reduced them eaſily into groſs pouder: *I* boiled this pouder no leſs then three times one after another, and every time *I* kept the heat half an hours time to the degree of evaporating the drop of Water in two ſeconds: and yet *I* was not able to melt the Horn: *It*'s parts do ſwell extreamly and become ſoft; but never quite Liquid enough to be caſt into a mould: however the ſame advantage can almoſt be had; becauſe they can be put into what ſhape one will, and being kept ſo well compreſt in drying, they do unite and make one continued body: The greateſt peices I have done in this manner were not above an inch over upon peices of Coyn, they would be poliſht perfectly well, and *I* See no reaſon to doubt but peices agreat deal bigger might be as eaſily made. *I* have alſo tryed the ſame thing with Horne raſpings ſuch as are found with thoſe that make horn Combs: But *I* was ſurpriſed to See this raſping did not at all appear to be ſweld nevertheleſs having put ſome quantity of it under a preſs till it were dryed, *I* found that it had ſo well united itſelf together that it had made a kind of Paſtbord hard and tranſparent, which I make no doubt would be uſeful to make ſeveral ſorts of Wares of.

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EXPERIMENT 14*d*.

As the Volatile Salts of Vipers have great virtues in Phyſick, ſo a great quantity of them are loſt in making the Vipers ready in the open Aire, Doctor *Guide* beleived then it would be advantagious to ſttewe them in the Digeſter; he brought me one day ſome Vipers dryed, upon which I made the following Experiments. I did ſhut up in a little Glaſs a drachm of dried Vipers, and four drachms of Water: I did put them in the Digeſter, and encreaſed the heat, till the drop of Water did evaporate in three ſeconds: I found the Bones of the Vipers extreamly ſoft, but the Liquor turned not into a Gelly, that made me believe the heat had been too great, tho' there was not any ſmell or taſt of Empyreume. I made then the ſame Experiment again; but this time I did put a little leſs Water in proportion to the Vipers; and encreaſed the heat alſo, but to make the drop of Water evaporate in five ſeconds, and by that means I had Gelly pretty ſtrong.

EXPERIMENT 15*th*.

As the new Digeſter may afford a very great quantity of Gelly, I have endeavoured to find out ſome new uſe it may be imployed to, beſides thoſe I have ſpoken of in the firſt Treatiſe of the Digeſter: I have then tryed to make uſe of it to take the Impreſſion of Medails and Seals, and I have ſeen that in melting ſtrong Gelly, and ſpreading it whilſt it is warm, upon ſome Medail or Seal, it hardens and keeps the print with all the exactneſs that can be whiſhed for; I have alſo made with it a thing, that ſurpriſeth at firſt, it is that having put ſome Gelly, in that manner, up-

on a very ſmall Seal, I took it away as ſoon as it was cool enough, and left it to dry; in drying it diminiſhed extreamly: But all parts in proportion; ſo that the Engraving remained ſtill very good, only it was ſo delicat, that it could not well be diſcerned without the help of a magnifying Glaſs: I ſhewed it ſeveral Perſons, that could never imagine, how it had been poſſible to engrave ſuch delicat Stroacks.

EXPERIMENT 16*th.*

I have alſo taken the Impreſſion of great Medails; but it was needful to let the Gelly dry quite upon the Metal: Becauſe, otherwiſe the peices being ſomewhat large, do warp in drying, if they are not applyed upon ſomething that keeps them ſtraight: I could alſo apply Silver or Gold to the concave part of that Gelly, which had been applyed to the Medail, and this Guilding appearing through the tranſparent Gelly, did repreſent the peice, as exact and ſharp, as the Original it ſelf: But I have obſerved, that by length of time it appears that the Gelly continues ſtill inſenſibly to dry and to ſhrink: So that the Frame into which thoſe Madails had been put in at firſt becomes to Large. The moiſture alſo will make them warp: So that I believe, that to preſerve them well, it would be needful to have little Veſſels made on purpoſe to ſhut them *in vacuo*, by means of a glaſs Place, where the Medail could be very well ſeen.

EXPERIMENT 17*th.*

I have alſo tried if Gelly ſeaſoned with ſugar, would not be fit to preſerve raw Fruit: I imagined that filling exactly

ly with very ſtrong Gelly, the Glaſs where the Fruit ſhould be, and then making the cover very exact upon the Glaſs, and faſtning it with Screws: I imagined, I ſay, that by this means, the Gelly having no communication with the Air, would not grow muſty; and that the Fruit which would be ſhut in it, as in a piece of Amber, would alſo remain ſtill in the ſame condition: Experience hath not anſwered my hopes. For Gelly is a matter ſo fit to ſuck up moiſture, that the Fruit it Surrounds communicates to it very quickly all it's Juice: By that means the Gelly ſwells, and the Fruit ſhrinks extreamly: Nevertheleſs, as the whole remains ſtill ſhut up in the Glaſs, and that the Gelly gets all that the Fruit looſes: I beleive this kind of Experiments will nevertheleſs be of a reall uſe, by giving us the advantage of making uſe of thoſe Fruits, which are not then regarded, by reaſon of their too great plenty, and by preſerving them for other Places, and other Seaſons, where a very good uſe may be made of them, both for Health, Food, and Curioſity. It is Plain alſo, that Obſervations of this kind, give ſtill ſome light to advance further into the knowledg of Nature: So that I hope it will not diſpleaſe, to ſee the chief Experiments I have made upon this Subject, tho' they had not always all the ſucceſs which *I* could wiſh for.

The firſt Fruit I did ſhut up in that manner was *Strawberries*. The 22*th*. of *July* 1 8 5 I fill'd a Glaſs with them about half full: I took afterwards Gelly that had about half it's weight in Sugar, and that was evaporated till there appeared a *Pellicule* or thin Skin upon it: This Gelly being come to have but as much heat as was neceſſary to keep it Liquid: I fill'd with it as exactly as I could, the Glaſs where the *Strawberries* were, and having applyed the Cover very exact upon it, I faſth'd it with a Screw: I kept this Glaſs about 8 Months time, in that manner; and the 24*th*. of *February* 1 8 I opened it: The Gelly was not impaired, and the *Strawberries* had preſerved their colour, but they had

 loſt

oſt almoſt all their Juice, which had paſſed into the Gelly and had rendred it much more pleaſing than without it.

EXPERIMENT 18*th.*

The 23*th.* of *June* I couered *Strawberries* and *Rasberries*, both raw with Sugar'd Gelly, about the thickneſs of a Crown peice, and I left them in the open Aire: a fourth night after I found them much dryed; but yet they were good.

EXPERIMENT. 19th.

The 23*th.* of *June* I fill'd up a Glaſs full of *Strawberries*, ſo that the Gelly which I put in afterwards, found place but in the *interſtices*: I did ſhut it up very exactly, and faſtened it with a Screw; but it hindred not the *Strawberries* being in ſo great a quantity, to drive out ſome part of the Gelly by the ſtrength of the Aire they produced in a fourth night; they were very good tho' a little withered, and the Gelly had alſo a perfect good taſt of *Strawberries*.

EXPERIMENT. 20*th.*

The 18*th.* of *July* I fill'd two Glaſſes almoſt full of *Strawberries*, and put Gelly ſeaſoned with Sugar, to fill up the *interſtices*: I ſhut theſe two Glaſſes with very good covers, well faſtened with Screws. The 10*th.* of *September* I opened one of thoſe Glaſſes, which had produced a great deal of Air, and driven out a great deal of it's Gelly; but that which

which remained had a very good taſte of *Strawberries*; but the *Strawberries* had almoſt quite loſt theirs. The other Glaſs having been better ſhut, was kept till the 19*th.* of *Aprill* of the following Year; and then having opened it I found that, tho' nothing almoſt had gone out from that Glaſs, nevertheleſs it had fermented, and the *Strawberries* given a very good Wine taſte to the Gelly.

EXPERIMENT 21*th.*

The 9*th.* of *September* I ſhut up *Strawberries* into two Glaſſes, which would not have held more, and having fill'd up the *interſtices* with Gelly ſeaſoned with Sugar, I applyed to them very exact covers, and made them firm with Screws: The 13*d.* of *January* I opened one of thoſe Glaſſes, and the Gelly was found to have taken almoſt all the taſt out of the *Strawberries* which were found dried; but not rotten. The 14*th.* of *February* I opened the other Glaſs, of which the *Strawberries* were found hard, and to have ſome ſmall ſowre taſt, but the Gelly was pleaſing, tho not ſo good as that of the *Strawberries*, which had been ſhut up the preceding Month.

EXPERIMENT 22*th.*

The 10*th.* of *Octo'er* I filled up to the top two Glaſſes with *Strawberries*, and having fill'd up the *interſtices* with Gelly ſeaſoned with Sugar: I made the cover firm with Screws. The 8*th.* of *February* I opened theſe Glaſſes which had loſt moſt of their Gelly; becauſe of the Aire that had been produced: The *Strawberries* and the Glaſſes were both Sowre, but nevertheleſs might be eaten; tho' they were not

not ſo good by much, as thoſe that had been ſhut even in the Month of *September*.

Theſe two laſt Experiments ſhew, that the *Strawberries* which are brought up by Art in the latter ſeaſon, have much leſs Spirits, and are much more ſubject to become Sowre, than thoſe that ripen in the Spring of the Year: For the *Strawberries* of the Month of *September* and *October*, had been much more ſpoiled in three or four Months time, than thoſe of the Months of *June* and *July* were in ſeven or eight.

EXPERIMENT. 23*d.*

The 25*th.* of *June*, I filled a Glaſs almoſt full with *Rasberries*, and then filled it up quite with melted Gelly: I ſhut it up quite with its Cover, and faſtened it with ſcrews as uſual: I kept it but three weeks, and the *Rasberries* appeared already leſs freſh, becauſe they had given much of their Juice and of their taſt to the Gelly; they had produced no Air.

EXPERIMENT. 24*th.*

The 18*th* of *July*, I filled a Glaſs almoſt full with *Rasberries*, and fill'd and ſhut it up as uſually. The 5*th.* of *September* I opened the Glaſs, the *Rasberries* and the Gelly were found to have all a very good taſt, the Fruit having given a great deal to the Gelly, tho' it were not much more whithered than that of the preceeding Experiment; and there had been no Air produced.

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EXPERIMENT. 25th.

The 1st. of *Auguſt* I repeated the preceding Experiment, but this time I kept the Glaſs ſhut till the 24th. of *February* nevertheleſs the Gelly was found yet to have a good taſt, and a good ſmell of *Rasberries*; there had been alſo no Air produced, but the Fruit was hard and without Juice.

All the other Experiments which I have made upon *Rasberries* have ſucceeded after the ſame manner; and it may be concluded, that by ſhutting in that manner this Fruit in Gelly, all it hath of good may be preſerved a longer time, and with leſs alteration than in any other Fruit; becauſe other Fruits produce ſome Air, which ſhews that the different Principles they are made of, act one upon the other, and make a kind of Fermentation which alters them; whereas in the *Rasberries* there happens nothing like it, which gives us alſo the conveniency to ſpare the ſcrews, which are neceſſary to faſten the Covers upon the Veſſels where other Fruits be preſerved.

EXPERIMENT 26th.

The 2*th.* of *July*, I did ſhut up in a Glaſs *Cherries* with Gelly, which was not much evaporated: in two days time they did thruſt the Gelly out of their Glaſs, by the ſtrength of the Air they produced: I took them out then, and I found their taſt was altered, but yet very good, and the gelly was alſo become more pleaſing than it was before it had been ſhut up; ſo that this Experiment will ſerve to give taſt to the Gelly only in *Cherry* Seaſon; I repeated then the ſame thing the 2*th.* of *July*, but this time I made uſe of Gel-

ly

ly evaporated to a ſtrong Pellicule; I kept them ſo four Months, and the 25*th.* of *November* I opened the Glaſs, and the *Cherries* were found very dry, but nevertheleſs had as good a taſt, as if they had been dryed in the Oven: The Gelly being ſwelled with the Juice of the *Cherries*, had taken a very pleaſing taſt, and which is not ordinarily found in that Seaſon.

The other Experiments which I have made upon *Cherries* have made me ſee, as well as this, that when they are to be preſerved a long time, the Gelly muſt be evaporated to a Pellicule; and that except the *Rasberries*, it is one of the fruits which produceth the leſs Air being ſhut up in ſtrong Gelly.

EXPERIMENT 27th.

The 21*th.* of *July* I did ſhut up red *Currants* in Gelly that was not much evaporated; the fourth day they did already begin to thruſt the Gelly out of their Glaſs, ſo that I opened the Glaſs, and I found they were good, and the Gelly had a great deal of their taſt. The 8*th.* of *Auguſt* I repeated this Experiment, but this time I made uſe of Gelly evaporated to a ſtrong Pellicule; I kept them ſo above 10 Months. The 9*th.* of *June* of the following Year, I opened the Glaſs, and I found that the *Currants* had thruſt out part of the Gelly of their recipient; they appeared handſome and plump, but in eating them they were found to be almoſt nothing but Wind; the Gelly was very good, and had much taſt of this Fruit.

The other Tryals I have made upon *Currants* have confirmed me, that the Gelly they are to be preſerved in, muſt be much evaporated as well as for *Cherries*; and that they produce a great deal of Air.

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EXPERIMENT 28th.

The 1*ſt.* of *Auguſt* I did ſhut up *Apricocks* into two Glaſſes which could not have held more, unleſs they had been cut into pieces; the Gelly which filled up the Glaſs had been evaporated to a pellicule: Nevertheleſs, the *Apricocks* began the next day to thruſt ſome of the Gelly out of their Glaſſes, tho' they were made very firm; becauſe they produced ſoon a great quantity of Air: After ten days I opened one of the Glaſſes, and I found, that the *Apricocks* had a good taſt, tho' a little fermented, the Gelly alſo had the taſt of *Apricocks*. I kept the other Glaſs above ſix Months, and did not open it till the 10*th.* of *February*. About a quarter of the Gelly had been thruſt out; the *Apricocks* were withered, and had the taſt altered, but yet it was good, the Gelly alſo had a good taſt of this Fruit.

I muſt here take Notice before I go further, that the gelly of theſe Experiments was of the ſame, which I alſo made uſe of to ſhut up the *Rasberries*, of the firſt of *Auguſt*, Exp. 25. Nevertheleſs the *Rasberries* produced no Air at all in more than 6 Months time; and the *Apricocks* produced ſome next day; that ſhews, that ſurely this difference comes but from the different Nature of theſe Fruits, and may give ſome light to know it more perfectly, by diſcovering the cauſe of theſe *Phenomena*, which are not to be attributed to the Acidity of *Rasberries*; ſince *Currants* which have at leaſt as much Acidity, will nevertheleſs produce a great deal of Air.

EXPERIMENT 29*th*.

The 8*th*. of *Auguſt* I ſhut up two *Apricocks*, every one in a very ſtrong glaſs: ſo that I could faſten them much more ſtrongly, than thoſe of the preceding Experiment: The gelly was alſo evaporated to a Pellicule, ſo that being put in a good frame, they did not drive out the gelly untill a Month after they had been ſhut up: only they withered much: I kept them ſo above ten Months, and at laſt the 19*th*. of *June*. of the following Year, I opened them; I found they had produced but a very little quantity of Air: The gelly was firm, and had a good taſt of the *Apricocks*; but the Fruit was abſolutely withered, and was tough like Leather; it had no bad taſt.

EXPERIMENT 30*th*.

The 30*th*. of *Auguſt* I would try whether by cutting the *Apricocks* to fill up the glaſs with them more exactly, I could perſerve them better, and hinder them to looſe ſo much of their Juice: I did therefore put a great quantity of pieces of *Apricocks* in a glaſs until it would hold no more, and I filled up the *Intreſtices* with gelly evaporated to apellicule: I made the Cover ſo firm upon it, that the 10*th*. day I found it broken by the ſtrength of the Air that had been produced within; the *Apricocks* were very good, but they had loſt their firmneſs; the gelly alſo had more of the taſt of the *Apricocks* than any I have ſeen; but for this ſort of Experiments, one ſhould have glaſſes, or ſome other receivers very ſtrong, otherwiſe they break.

For

For fear of being too tedious, I ſhall not relate particularly all the Tryals I have done upon *Peaches*, *Nectorals*, and *Plumbs*, which are all Fruits with Stones, as well as *Apricocks*; I ſhall only impart ſome Obſervations which I have made upon them.

1*ſt*. *Nectorals* produce much Air, become very tough like Leather, and do not give very much taſt to the gelly: I have nevertheleſs obſerved upon one I had kept 10 Months, and which I opened in the month of *June*, that the gelly which remained in the glaſs was very pleaſing, but much more fluid than the gelly where *Apricocks* have been kept; there was alſo a white and hard Subſtance, ſticking to the ſides of the glaſs, which I took to be a kind of *Tartar*.

2d, *Peaches* produce much more Air than *Apricocks*, and render the gelly leſs firm; they are alſo very ſubject to become ſowre, unleſs there be a great quantity of gelly in proportion to the Fruit.

3d. *Plumbes* preſerve their taſt very well, and I have kept ſome *Bullaces*, which ſix Months after, tho' they were cut in pieces to fill up the glaſs more exactly, had almoſt produced no Air; they had kept their colour very well, and had a better taſt than when I ſhut them up, becauſe they did farther ripen, being kept; but they had alſo loſt their firmneſs. The gelly had drawn from them a pleaſant taſt.

EXPERIMENT 31*th*.

The 5*th*. of *September* I fill'd up a glaſs to the top with Verjuice *Grapes*, and fill'd up the interſtices with Gelly evaporated to a Pellicule: I kept this glaſs above 7 Months and a half, and did not open it till the 26*th*. of *April*, of the following Year: I found that a great quantity of gelly had

been thruſt out, and that which remained became very liquid, becauſe the Juice of the Verjuice had all got into it: So that the Fruit remain'd quite dry and withered. The gelly had a taſt as though it had been ſeaſoned with Wine.

It may be, that the Verjuice was a little to ripe at the very time I did ſhut it up; but it is certain alſo, that the Juice was grown riper by being kept in this manner, much more than if it had been kept otherwiſe; and that hath made me think of trying another Year what Effect will be produced by riper *Grapes*.

EXPERIMENT. 32*th*.

Barberies being a very ſour Fruit, which keeps very well in *Vinegar*, I did hope it would not ferment ſo eaſily as others do, and ſo would be fitter to be preſerv'd in gelly without making preſſure. The 7*th*. of *September* then I fill'd two glaſſes almoſt full with that Fruit, and fill'd them up quite full with gelly not much evaporated; it happened, that in a fourth Nights time, ſo great a quantity of the gelly was thruſt out, that I open'd them: The *Barberries* had given much of their taſt to the gelly, and were good ſtill, tho' they had ſome fermented taſt. The 11*th*. of *September* I repeated the ſame Experiment upon *Barberries*, but the gelly was evaporated to a Pellicule: The glaſs I had put it in was weak, and ſo I found it broaken the 14*th*. Day by the preſſure of the Air that had been produced within: The *Barberies* were already withered, and had communicated ſome of their taſt to the gelly, which made it more pleaſant.

This Experiment confirms what I have already obſerved, Exp. the 28*th*. that tho' Fruits be acid, they are not leſs fit

to

to ferment and produce Air, at leaſt when they are kept in gelly ſeaſoned with Sugar.

EXPERIMENT. 33*th.*

The 12*th.* of *September* I fill'd up quite a glaſs with *Quinces* peeled and cut into pieces, and I filled up the *interſtices* with gelly evaporated to a pellicule; in ſix days time I ſaw that the *Quinces* were already very much withered; ſo that I opened the glaſs, and I found that the gelly being ſwell'd up with the Juice of the *Quinces*, was become much more pleaſing. The 29*th.* of *September* I repeated the ſame *Exp.* upon a greater quantity of *Quinces* in a great glaſs, with intention to ſee, if by keeping them a much longer time ſome notable difference might be obſerved, I kept them then almoſt ſeaven Months, and did not open them till the 20*th.* of *April*, of the following Year; but I obſerved almoſt no difference from thoſe I had kept ſix days.

EXPERIMENT. 34*th.*

The 27*th.* of *September* I did ſhut up *Quinces* peeled and cut in pieces, in two Glaſſes with Gelly evaporated to a pellicule: but in one the *Quinces* had been boilled, and in the other were raw: Three weeks after I opened both Glaſſes, and *I* found that the raw *Quinces* were very withered, and had communicated much of their taſte to the Gelly; but thoſe which had been boil'd were not at all withered, and the Gelly had not almoſt taken any thing of their taſte.

I have made ſeveral other Experiments upon *Quinces*, but *I* ſhall not relate them; becauſe I have obſerved nothing in them, but what may be ſeen in the two preceding: That

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is, that raw *Quinces* give up ſoon their Juice to the Gelly, but afterwards they can be preſerved in it a long time, without any other alteration, and without producing much Aire: The other obſervation, and that I have alſo confirmed ſeveral times, is that boil'd *Quinces*, tho' much ſofter, ſuffer much leſs changes being ſhut up in Gelly than raw do, and it may be the ſame thing will happen to other Fruits.

I ſhall ſay here alſo, that ſeeing raw *Quinces* after they had ſtayed a long time in the Gelly, were become ſo hard and tough, that they were not fit to be eaten: I would try what effect they would produce being boil'd in the Digeſter, with ſome of the ſame Gelly they had been kept in, and I have always found, that it hath ſoftened them very well, and made them very pleaſing to be eaten.

I remember yet two Obſervations *I* have made upon *Quinces*: One is that their peeling being ſhut up with Gelly, gives it almoſt no taſte; whereas to ſome other Fruits, it is the thin outward peeling which gives more taſte than all the reſt of the Fruite: The other Obſervation is, that the peices of *Quinces*, as long as they are ſhut up in the Gelly, keep very well their colour yellowiſh; but as ſoon as they are expoſed to the Aire, they become of a dark red colour very quickly.

EXPERIMEMT 35*th.*

The 14*th.* of *October I* did ſhut up ſome *Orange* peels in Gelly, that was not evaporated quite to a pellicule: *I* kept them till the 4*th.* of *November*, and then *I* opened them: I found that the inward preſſure had thruſt out ſome of the Gelly, and the *Orange* peelings had given it too much of their taſt: So that it was needful to mingle it with other Gelly to abate that taſt and render it pleaſing. I would afterwards ſee how the inſide of the *Oranges* would do, and the

24*th*

24*th.* of *November* I did ſhut up a *Chiney Orange* peeled and parted in quarters: I covered it with Gelly evaporated to a pellicule. The 11*th.* of *January* I opened that Glaſs, and I found not that any Aire had been produced. The Gelly had a great deal of the taſte of the *Orange*, and was very pleaſing; but the Fruit had almoſt loſt all it's Juice; it had nevertheleſs a good taſte.

This Experiment having made me beleive that the rinde hath ſomething fitter for fermentation, than the inſide of the *Orange*: *I* have made ſome other Tryals, which have confirmed me in that Opinion, and which *I* believe it would be needleſs to bring here: *I* have alſo ſince that time tryed that *Chiney Oranges* and *Lemons*, being ſhut up whole with their rind on in Gelly evaporated to a pellicule, give it almoſt all their Juice, tho' one ſhould think that the thickneſs of the rinde ſhould hinder that communication. The Gelly where *Chiney Oranges* have been preſerved, hath always appeared to me more pleaſing than that of the *Lemons*: And both the one and the other did produce ſome Aire, being ſo ſhut up with their rind on.

EXPERIMENT. 36th.

The 20*th*, of *September* *I* did ſhut up ſome *Pipins* peeled and cut in pieces in a receiver fill'd up with Gelly ſcarce evaporated to a pellicule: The 20*th.* of *October* *I* opened this receiver: The *Apples* had produced very little Aire, and they did not at all appear withered as the other Fruits had done: So that there was reaſon to hope for a good ſucceſs; but when *I* taſted them *I* found them extreamly altered and ſpoiled. This effect appeared to me ſo ſurpriſing, and ſo differing from all thoſe *I* had already ſeen, that *I* had the Curioſity to ſee if it would always ſucceed ſo: So that the 15*th.* of *October* *I* did ſhut up ſome other pieces of *Apple*

ples in Gelly evaporated to a pellicule, and I took care to put but a little quantity of Fruit in proportion to the Gelly, to take away the ſuſpition that might have been, that there was not Gelly enough to ſuck up the Juice of the *Apples*. I did ſhut up alſo at the ſame time ſome pieces of raw *Quinces* in another Glaſs with ſome of the ſame Gelly, and after I had kept them 25 days I open'd them and found that the *Quinces* were withered, and had given their taſt to the gelly; but the *Apples* appear'd freſh yet, as at the other time, and had an unpleaſing taſt of ferment; but had given but very little of it to the Gelly. I do not believe then that this Experiment can be uſeful to preſerve *Apples*, but this hath not hindred me from putting it here: becauſe it can furniſh matter to reaſon upon the cauſe which hindered the Juice of peel'd *Apples* from entring into the Gelly, ſince it is ſo fit to ſuck up the Juices of other Fruits, that even *Oranges* and *Lemons*, tho' encloſed with a thick Rind, do dry and wither in the Gelly, as we have ſeen in the preceding Experiment. I had already obſerved Experiment the 24*th*. That boil'd *Quinces* do not wither in Gelly; but I was not ſurpriſed at it, becauſe they do not ferment in it.

EXPERIMENT. 20*th*.

I have been willing to ſee how *Flowers* would be preſerved in Gelly: For that end the 10*th*. of *September* I did ſhut up five *Roſe Buds* already a little withered in Gelly ſcarce evaporated to a pellicule. Ten days after I perceived, that the *Roſes* began already to produce ſome Air, and lift up their Cover: This did not hinder me from keeping them the ſpace of eight Months; but the 20*th* of *April* of the following Year, I took them out, and found that they had loſt a little of their Colour, and were fermented, ſo that they had given a taſt of Wine to the gelly, which was very

pleaſing

pleaſing, and which had alſo the ſmell of *Roſes*. The 12*th*. of *Sept*. I would make the ſame Experiment with Violets; but the glaſs was ſo weak, that I found it broken 10 days after, by the ſtrength of the Air produced by the Violets; the Gelly had a good taſt of the Flowers, and they were not ſenſibly changed; the ſame day 12*th*. of *Sept*. I ſhut up Gilly-flowers, and ſome little Flowers, in ſome of the ſame gelly that was not yet evaporated to a Pellicule; I kept them till the 10*th*. of *March*, of the following Year; and then I open'd the glaſs: I found that the Gilly-flowers had loſt a little of their Colour, and that they had thruſt out a great deal of their gelly; but that which remain'd had a very pleaſing taſt, partaking of the taſt of gilly-flowers, and of that of Wine.

EXPERIMENT 38*th*.

Theſe Expiriments we have juſt now ſeen upon three differing ſorts of Flowers, ſeem to prove that the Flowers have very active Principles, and fit to make a Fermentation, which produces Spirits of the ſame Nature as thoſe of Wine: Nevertheleſs, I have tryed that Hares-bell is not ſo; for I have kept many of thoſe Flowers in a little glaſs with gelly ſeaſoned with Sugar ſince the Month of *April* 1685, till the Month of *June* 1686, and yet I have not perceived they had made any Fermentation, but they did always appear freſh; only it ſeem'd, that the blew Colour had gathered together in divers little heaps, in the ſame manner as Oyl or greaſe, which ſwim upon Water, are ſeen to gather together in ſeveral little drops.

Having declared my Manner to preſerve Fruits and Flowers in gelly; I muſt take Notice of a defect which hath offended ſeveral Perſons; it is that the gelly which hath been ſo kept for a long while, hath ſome ſtrong ſmell, which yet

is not obſerved by the taſt ; but in Experiments, things are always obſerved by the ſmell before they be put in the Mouth ; ſo that many Perſons have found ſomething which did not pleaſe them : I have therefore tryed ſome Experiments to remedy it.

EXPERIMENT. 39.

The 23*d.* of *April* I took ſome raw Quinces, which I kept in gelly ſince the 21*th.* of *Sept.* of the preceding Year : I did put ſome of them in three ſeveral Veſſels with Wine : I put one of thoſe glaſſes to boil in the open Air ; I exhauſted all the Air of the ſecond, and ſo made the gelly boil *in vacuo*; and I ſhut up very exactly the third, to give leaſure to the Wine, to penetrate it without heating it; it was found that the Wine did very well correct the defect we are ſpeaking of; but eſpecially that which had been boiled *in vacuo* was found beſt : So that this Remedy might ſerve for thoſe which are dainty, and even Cinamon, or ſome other ſweet ſmell might be added to it : But there will always be three times as many People to whom that will be needleſs, and who eating the gelly, ſuch as it is taken from the glaſs, will find it very pleaſing, and will be glad to make uſe of it, being a thing very noriſhing, and eaſie to be digeſted.

In the mean while, the ſmell we ſpeak of, ought not to make People ſhy, as if this gelly were hurtful : This ſmell comes only from the Quantity of volatile Salts which are in the gelly, and which may be a little exalted : It is known how much the Spirits or volatile Salts of *Harts horn*, and others of the ſame *Species*, are eſteem'd in Phyſick, to purifie the Blood, and to reſtore Nature ; ſo that far from fearing the uſe of the gelly, becauſe of the ſmell we ſpeak of ; it ought rather to be eſteemed the more, as being fit to produce great benefits towards our Health, provided it be not uſed with exceſs. I ſhall ſay beſides that, in many Caſes the

gelly

gelly hath a very good ſmell as when *Rasberries* or *Bullaces*, *Roſes*, *Gilly-flowers*, *&c.* have been kept, ſo that here, even Perſons very dainty will find nothing unpleaſant: I believe then, that this manner of preſerving Fruits is better than any of thoſe that have been already made uſe of; as well for the cheapneſs, as for preſerving well the taſt of the Fruit.

Having thus ſpoken of the uſes which can be made of the gelly, I think I ought alſo to ſay the means I have made uſe of to make it fine and eaſily, and the quantity of Sugar I put in it. I ſhall ſay then, that the Engine which I make the oftneſt uſe of, containeth about one pound of *Harts-horn* or *Ivory* ſhavings with above ſix pounds of Water, and there remaineth about a twelv'th part emptyed; I encreaſe the heat to evaporate the drop of Water in 4 or 5. Seconds: I take out afterwards this matter, which being ſqueezed through a Cloth all the Liquor is preſt out as much as may be, and that Liquor is all a ſtrong gelly, which weighs above five pounds; a little leſs than a pound of Sugar is put into it; it is put upon the Fire till it begins to boil; and then the Fire is to be taken away, ſcum the gelly, and leave it to make a ſediment: It is convenient not to remove all the Fire at once, that ſo the gelly may coagulate by degrees, and have more time to let fall its ſediments; when the gelly is cold, it is clear enough to ſee clearly through it, the bottom of the pan tho' the gelly were three or four inches thick; ſo that for two penny worth of *Ivory*, and four penny worth of Sugar, one may have between 5 or 6 pounds of very good gelly, and pretty fine, and the *Ivory* may ſerve again to make other gelly, as I have obſerved *Exp.* the 5*th.*

If afterwards one deſireth to employ this gelly to preſerve Fruits; he muſt take the upper part which is clear from the ſediment which is at the bottom, and having put it in an earthen Pan, evaporate it upon a ſmall Fire, till a kind of a little skin be found to ſtick to the ſpoon when it is ſcum'd; then this gelly muſt be left to cool till there remains to it but as much Liquidity as is needful to make it run into the interſti-

ces of the Fruits or Flowers that are to be preſerved with it, then the glaſs or gelly pot wherein the Fruits or Flowers are contained being fill'd up exactly with the Gelly, the cover muſt be applyed to it, theſe Frames as I have obſerved Experiment 22*th*. would not be always needful, but however they may be had very cheap, eſpecially if they be made in quantities. Now tho' the Gelly become too much Sugar'd after it is evaporated to a Pellicule, it doth no hurt: becauſe afterwards, ſwelling by the Juice of the Fruit, it makes a ſweet Meat, which is ſtill much leſs Sugar'd than ordinary Sweet-meats.

The manner deſcribed above for making Gellys is very good for the ordinary Uſe; but becauſe people may ſometimes be deſirous to have Gellyes of an extraordinary Fineneſs and Tranſparency: I have alſo Search't out ſome means to make them eaſily, and knowing that filtration thro' cap Paper, is one of the beſt means to have Sugar very pure, but the tedious time required for the Operation is the reaſon that it is not ordinarily made uſe of, I have endeavoured to remedy this inconveniency, and for that end I have made uſe of the following Engine.

A A is a Glaſs Receiver.

B B a Cover exacty fitted to it.

C C C. a Pipe which maketh the communication between the Receiver A A, and the Aire Pump.

D D. a flat Veſſel whoſe bottom is pierc't with quantity of ſmall holes.

E E E. a Pipe which maketh the communication between the Veſſel D D, and the Receiver A A.

F F. a Veſſel which containeth the Liquor to be filtrated.

G G. the Plate of the Aire Pump.

To make uſe of this inſtrument, the Veſſel DD muſt be well tyed with linnen Cloth and afterwards with cap Paper: So that the Liquor may not get into it but thro' the cap Paper and the Linnen: The Veſſel F F being afterwards fill'd with the Liquor to be filtrated: and the Aire Pump Exhauſting the Aire

Air from the Receiver A A, the Liquor in the Veſſel F F is neceſſarily driven through the cap Paper, and the Linnen into the flat Veſſel D D, and from thence into the Veſſel A A, through the Pipe E E E, the great ſtrength wherewith the Atmoſphere preſſeth upon the Liquor of the Veſſel F F makes it paſs incomparably faſter than it would in the ordinary manner, and the Linnen hinders, that this ſtrength tears not of the Cap Paper.

This Inſtrument hath pretty well ſucceeded, yet becauſe by reaſon of the difficulty of governing the Engines, it is not ſo fit for general uſes: I have endeavoured to produce an effect near to that, by means of another Inſtrument which might eaſily be made uſe of by any ſort of People, and to that end I made the the following Engine, *vid.* Fig. 10.

A A is a kind of a Ring made of Pewter, having both its apertures ground very flat.

B B. C C, are two other Rings alſo of Pewter, having every one one of their apertures ground, to wit, that which is to be applyed to the Ring A A; and upon that aperture they ought to have every one a Linnen ſpread and tied about the Ring: and upon this Linnen there muſt be alſo upon every one a piece of Cap Paper ſpread as broad as the Linnen.

D D D is a Frame to encloſe the three Rings A A, B B, C C, being applyed the one to the others, as is expreſt in the Sheme. E E, is the ſcrew to preſs ſtrongly the three Rings one againſt the other, that nothing may paſs between them; but that the Liquor powred into the Ring A A, may be forced to paſs through the Cap Paper, and the Linen put betwixt the Ring A A, and the two others.

F F, a Funnel ſoddered upon a hole of the Ring A A.

The Liquor to be filtrated, muſt be powred into the Funnel F F, till the Ring A A, and the Funnel alſo be filled with it; by that means the Liquor which is in the Ring is preſt by the whole heighth of the Liquor in the Funnel, and as it may be made very high, it may alſo make a very conſiderable preſſure upon the Liquor which is in the Ring,

and

and therefore this Liquor may be forc't to paſs very quick through the Cap Paper and Linnen Cloth we have ſpoken of, and this Engine being fitted ſo as it is, hath the conveniency to be very eaſily taken to pieces, and furniſhed with new Papers.

Tho' I have Experimented that for the purification of Sugar, it is better to make uſe of the manner deſcribed *Sec.* 2. 21*th*. It hath not hindred me from putting here theſe two inſtruments, becauſe they may be ſtill of uſe to filtrate other Liquors, to which Pipe Clay might not do the ſame effect as it doth upon Sugar.

Having Sugar well purified of what manner ſoever, you muſt take *Lemon* Juice that hath been kept by the manner deſcribed *Sec.* 2. *Exp.* 23*d*. this Juice is clearer than it can be made by filtration, and it is eaſie to have always proviſion of it: One may alſo have eaſily very pure Gelly if it is made in the Digeſter of the new faſhion; becauſe as one hath the conveniencie to ſhake it, it is not needful to put ſo great a quantity of Ivory in proportion of the Water: The heat muſt be encreaſed to evaporate the drop of Water in 5 ſeconds: Then this Engine being ſet upon the ground with its Cover upwards to let it cooll, it happens that whilſt the Engine is hot the ſediment falls to the bottom: So that afterwards when it is opened a good quantity of very clear Gelly is found in the upper part: having ſo all the materials very clear and fine, there remaines but to mingle them in the quantity which is Judged beſt by the taſt, as about 15 parts of Gelly, 6 of Sugar 4 of Wine, one of *Lemon* Juice: They muſt boil a little together and then be poured into the Pots: by that means one may have very fine Gelly without much trouble.

The method I have given to Clarify being fit only for matters very liquid, and where there is great quantity of humidity to be evaporated; it hath given me occaſion to think alſo of ſome way how to facilitate the evaporation: I have then imagined, that the worm which hath been made

made uſe of a long time in *Chymiſtry* to facilitate the Condenſation of Spirits; might be as well applyed to heat the Liquors to be evaporated, and that ſo this Operation might be done quicker, and with leſs Fire: I have then made upon that Subject the following Experiment.

EXPERIMENT 40*th*.

I took a Veſſel ſuch as is repreſented *Fig*.11 having a worm inſerted in a hole in the middle of the bottom: So that this worme might ſerve as a Chimney Pipe to the Fire that ſhould be put under the Veſſel: I filled the ſaid Veſſel with Water and put underneath a Lamp which had a great wicke lighted: I ſuſpended at the ſame time a *Thermometer* in the Water, and a bout 12 minutes time were required to raiſe the Liquor in the *Thermometer* from the 15*th*. degree to the 2 *th*. I took of afterwards the worm, and I ſtopt exactly with a metal plate the hole where it had been inſerted: I put in again afterwards new water into the Veſſel, and I put a pound more than there was the firſt time: becauſe the Worm I had taken of did alſo weigh a pound: I left the *Thermometer* ſuſpended in the Water as the firſt time till it was returned to its heighth of 15 degrees: Then I put the ſame Lamp at the ſame diſtance under the Veſſel with the ſame wicke equally ſnuft, and the ſame quantity of Oyl: and this time there were twenty minutes and a half time required to raiſe the Liquor in the *Thermometer* to the ſame heighth of 21 degrees, where it was before mounted in 12 minuts time: Now the worm I made uſe of in this Experiment made but one turn, from whence it may be judged that thoſe which ſhould make ſeveral turns would cauſe a much greater difference. Seeing then this invention may be ſo ſaving in ſeveral Trades, whoſe chief Expence conſiſteth in evapo-

ration,

ration, I think I have not done a miſs to have inſerted it here.

I have not been contented to improve the Digeſter for Meats only, I have alſo workt upon that which I imagined would be moſt fit for Diſtillations: I only gave the Deſign of it in the 5 *Fig.* of the firſt Treatiſe of the Digeſter *Chap.* 6. *Exper.* 3. but ſince that I have effectively made it up with ſome alterations, which may be obſerved in the following Deſcription.

Vid. Fig. 12. A A is the great hollow Cylinder to contain Water.

B B is the inward pot to contain the matters to be diſtilled.

C C is a Ring of caſt Braſs very thick, with a hole in the middle, through which the pot B B paſſeth, and is very exactly and ſtrongly ſoddered to it.

D D an Iron Ring very ſtrong, which ſurrounds the aperture of the Pot B B, and is applyed upon the Circle C C.

E E two Appendices or Trunions of the Ring D D where with the ſaid Iron Ring being made firm with Iron Pins, to the Iron Frame G G G G and being drawn by means of the ſcrews F F: It preſſeth very hard the Circle C C, and maketh it ſhut exactly the aperture of the Cylinder A A.

H H a Veſſel whoſe aperture is applyed very exactly to the aperture of the pot B B: this Veſſel may be ſcrved very cloſe to the ſaid Pot, by means of the Screws L L, and it may be made up of Glaſs or Metal.

M M M a ſmall Pipe with a ſtop Cock, through which the Air may be exhauſted from the Pot B B when one deſires to diſtil *in vacuo.*

N N two *Appendices* to the Iron Frame to keep up the whole Engine ſuſpended, as is ſeen in the Scheme; to the end it may eaſily be turned upſide down.

The

The ſpace between the great Cylinder A A, and the inward Pot B B muſt be fill'd up with Water; and the matters to be diſtill'd being put in the pot B B, ſome metal Grate muſt be put in it to hinder them from falling down, which yet gives Liberty to the Vapours to paſs into the Veſſel H H: the Engine muſt afterwards be turn'd, ſo that the Veſſel H H being downwards, the Vapours may remain in it when they are condenſed: The Fire muſt be applyed to the Cylinder A A; and it may be upheld there, by means of a ſemi-circular Iron Plate hung upon the hinges O O: a Veſſel full of cold Water may alſo be had, into which the lower part of the Engine is immerſed, to the end the vapors may be condenſed quicker in the Veſſel H H. By that means it may be tryed what different effects will be produced in the diſtillation when it ſhall be made in different degrees of rarefaction and condenſation of Aire: And diſtillations may be made in *balneo Mariæ* quicker, and in more difficult caſes than uſe to be done; and there will be no danger to looſe any of the more ſubtile and more volatile parts: this Engine doth very well it's effect, and I have already done ſeveral Experiments with it, of which I ſhall relate only theſe two or three.

EXPERIMENT. 41*th*.

I ſhut up in the Engine above deſcribed ſome *Lemon* peels, ſo that the pot A B was almoſt quite full of them, I put a grate to hinder them from falling, and having applyed the Pot H H, which was of Glaſs, I exhauſted the Air through the cock M M; ſo that the *Lemon* peels were *in vacuo*: I ſhut up the cock, and having turned the Engine upſide down, to the end the Veſſel H H might be downwards, I applyed the fire to the Cylinder A A, and having encreaſed the heat to evaporate the drop of Water in 40 ſeconds I

kept it at that heighth about an hours time: I found afterwards, that the peels had given a pretty good quantity of Liquor, which had absolutely the same smell as the peels from which it had been drawn: So that it was not perceived that the Fire had made any alteration: There was also Oyl which swim'd upon that Liquor, but I did not seperate it; and I saw two day's after that there was a kind of sediment, which gathered to the bottom, and which I believed was made up of the grosser Particles separated by the distillation, which uniting together did become so heavy, that they fell to the bottom, Ihave kept this Liquor above two months, and it hath always had a very sweet smell: The quickness wherewith it had been distilled, together with the natural smell it had kept without being altered by the Fire, gave Opinion that this Engine would be very useful.

EXPERIMENT. 42*th*.

Another time I did shut up four pounds of green *Goosberries* in the same distilling Engine, and I put the Vessel H H, of Metal and did shut it strongly with screws, to the end it might beare the pressure the heat was to cause in it: for this time I took out no Air: I increased the fire to evaporate the drop of water in 10 seconds and then took part away and left but as much as was needfull to keep the heat to evaporate the drop of water in 20 seconds: after half an hours time I took the fire quite away and the engine being cold I found about two pounds of a thick Juice extracted from the *Goosberries*, and they were boil'd to a good degree.

Ex-

EXPERIMENT. 43*th.*

Soon after the Experiment above, I did ſhut up an equal quantity of green *Gouſberries* in the ſame Engine; and having extracted the Aire that was ſhut up with the Fruit, I encreaſed the heat to evaporate the drop of Water in four ſeconds; then I took away part, and left but as much as was needful to keep the heat to evaporate the drop of Water in 20 ſeconds, and after about 6 hours time I took it quite away: The Veſſels being cool'd I found that about three quarters of a pound of a Juice, much thinner than that of the preceding Experiment, had been diſtill'd: So that I ſaw that in 5 or 6 times more time there had been leſs by half of Liquor diſtilled *in vacuo* than in the compreſſ't Aire: This *Phænomenon* ſurpriſed me ſo much the more, for that I had ſeen at *Venice* ſome effects abſolutely contrary to it, the diſtillation having been always quicker *in vacuo* than in the ordinary Aire; but my wondering ceaſed, when I reflected upon the cauſe of theſe Experiments, which may be ſeen *Sec.* 3. *Exp.* 32 where the reader will be ſatisfied that theſe *Phænomena* do not really contradict one another; and there will be ſeen at the ſame time the ſeveral circumſtances, wherein theſe new ways of diſtilling may the one and the other, be applyed with conſiderable advantages.

SECTION

The SECOND.

Concerning ſome IMPROVEMENTS, *and new* USES *of the Air* PUMP.

IT is now about twelve Years ſince I publiſhed a new Aire Pump, which was pretty well received, and which hath been put in uſe by ſeveral Perſons ſince that time: that ſmall book was ſold at *Paris* by *Jean Cuſſon* and had the title of *Nouvelles Experiences du Vuide avec la deſcription des Machines qui ſervent a les faire*: But as theſe little Works are ſubject to be loſt being not uſual to reprint them, I believe it needful I ſhould repeat here part of the deſcription which I gave then, to the end ſome conſiderable alterations I have made to it ſince that time may be well underſtood, *Vide Fig.* 13.

A A A is the Frame with three legs two foot and a half high.

B B The body of the Pump very even from one end to the other, made firm upon the ſaid Frame by three Screws which paſſe thro' three peices of Braſs ſoddered to the ſaid Pump.

C C Is the Cover of the Pump placed ſloping wiſe.

D a hole in the higheſt place of that cover, and ſhut by a Valve which lets the Aire go out of the pump and hinders it from coming in again that way.

E E a shallow vessel soddered upon the cover C C: which serves to keep always water upon the valve D.

F: a little hole stopt with a peece of bladder and a brass wire, through which the Aire may be let in into the receiver again.

G G The plate of the pump upon which is applyed the receiver V V.

H H a pipe soddered by one end to a hole in the middle of the plate G G; and by the other end to the middle of the cover C C: to the end there may be a communication betwixt the receiver V V, and the Pump B B.

I a Valve fitted to the bottom of the Pipe H H, to let the Aire pass from the receiver V V into the Pump B B, and hinder it to return the same way again.

L L a small Pipe soddered towards the top of the Pump, and stopt with Leather tyed strongly round about it.

M M a Brass Wier that passeth through the Leather that stops the small Pipe L L, and reaches to the Valve I, that one may, by moving the external part of the Wire, open or shut the Valve I as need requires, without letting the external Aire get in through the pipe L L, because the Leather stops it exactly.

N a little weight to counterpoise the valve I, and keep it shut.

O O an Iron rod pinn'd to the plug of the Pump, having at the lower end a stirrup in which puting the foot one may move the Plug up and down: this Iron rod passeth thro a hole in the center of the Ball Q, which serveth to guide it, that it may move directly according to the Axis of the Pump.

R R S S is the Plug of this Engine; it differs so much from other Plugs, that every part of it must be explained: It hath two chief parts, R R and S S, the upper part is the longest, because it sustaineth all the weight of the Atmosphere, and is hollow within to contain Water; the other part S S, is shorter because it serveth only to uphold the Water contained in the part R R, and hinder it from falling

T T is a ſmall Pipe open at both ends; it paſſeth thro' the part S S, to which it is ſoddered, and reacheth till near the cover of the part R R.

X X is another Pipe which paſſeth thro' the middle of the part S S, to which it is ſoddered, as well as to the bottom, which covers the part R R; that Pipe hath a hole Y near the ſaid cover.

When the Plug is at the bottom of the Pump, the ſmall end of the crooked Pipe repreſented *Fig. 25th.* may be inſerted into the end of the Pipe T T, and pouring Water into the funnel at the top of the ſaid crooked Pipe, this Water muſt go thro' the top of the Pipe T T into the upper part of the Plug, the Aire going out of it freely by the hole Y, thro' the lower aperture of the Pipe X X; the Water fills up then alſo the ſpace Z Z, which is betwixt both parts of the Plug, and this Water can't fall, becauſe the part S S hinders it, the part R R is then all ſecured with Water, and the Air cannot get betwixt the Pump and the Plug into the *vacuum*, which we make above it.

To make uſe of this Engine, Water muſt be powred into the Pump thro' the Pipe H H, that in thruſting the Plug towards the top of the Pump, this Water may fill up all the ſpaces where the Plug can't get. and that by this means the Aire may be all driven out of the Pump, thro' the hole which is under the valve D: afterwards the receiver to be exhauſted muſt be applyed upon the plate G G, and the ſmall weight N muſt be lifted up to open the valve L, at the ſame time that we draw the plug downwards with the foot; ſo it happens that the Aire of the receiver V V runs freely into the pump B B through the pipe H H and then we ought but to let the valve I ſhut again to hinder the Aire from returning the ſame way, and make it be forced to come out thro' the hole D when we thruſt the plug up wards: ſo that by repeating this Work the Aire may be exhauſted out of the receiver as exactly as can be expected by means of Engines; for when the Plug is drawn downwards

wards there remains no Aire in the Pump, and none can come from without to hinder the *vacuum* from being perfect: and the Aire of the receiver runs into it moſt freely, not only by reaſon of it's ſpringineſs, but alſo by it's weight; the ſmalneſs of the holes of the Cock in my firſt Aire Pump might cauſe ſome hinderance; for it is known, that Air doth not paſs freely thro' narrow apertures, but here there is no ſuch thing to be feared: For it is eaſy to make the Pipe H H more than ſufficient that the Aire may freely run through and this Pipe going directly downwards perpendicular without any horizontal turning, there is reaſon to hope that even when the Air of the receiver V V is dilated to that heighth that it can no more act by its ſpringy power to extend it ſelf equally in the receiver and in the Pump, it's weight is yet ſufficient to make it fall downwards, and ſo leave the receiver V V abſolutely emptyed of Aire.

I know that the Learned *Monſr. Sturmius* inthe ſecond volume of his *Theatrum experimentale* gives the deſcription of an Air pump which he pretendeth to be preferable in many occaſions to all others: but I do not ſee that that which he giveth us may have any uſes to which this may not be very well applyed, and even I'le make bold to ſay, that I think there wants yet ſome thing to his: he obſerveth himſelf ſome faults in it, but that which ſeemeth to me moſt conſiderable is in the valve which ſerveth to hinder the Aire from returning from the pump into the receiver again: for he hath very well obſerved that the ſpring applyed behind that valve, was very uſeful to ſhut it up well, but that alſo on the other ſide it made a great reſiſtance to the Air that was to come out of the receiver, ſo that when this Air was conſiderably dilated it was no more able to open the valve, and ſo the *vacuum* could not be made perfect in the receiver: Now he could not but know that there will be ſtill the ſame objection againſt the weight that he hath ſince put in place of the ſpring: For tho, this weight doth not reſiſt ſo much as the ſpring did, it doth yet make reſiſtance

ance enough to retain in the receiver a quantity of Air ſufficient to hinder a good ſucceſs in many occaſions; it is therefore much better to make uſe of the inſtrument which I have deſcribed: ſince it remedies ſo well all ſorts of inconveniencies.

'Tis not upon meer theory that I ſpeak ſo to the advantage of the Engine I have deſcribed: I have alſo made tryals of it which leave no place to doubt of the exactneſs of it's operations: For example it had been hitherto accounted for certain that Air was abſolutely neceſſary for the firing of combuſtible matters; but as for the fire made by the colliſion of two hard Bodies, as a Flint and and a Steell, it was not believed that Air hath any part in that effect; and indeed when the Experiment of it is made with Engines that are not very exact, we ſee that the Flint and Steell of a Piſtol in the receiver exhauſted, produceth ſparks in the ſame manner that it would do in the open Air; but I have tryed that if the receiver be exhauſted by means of the Engine deſcribed here above; this *vacuum* is ſo exact that if you endeavour to fire the Piſtol in it, there is not the leaſt ſpark of fire to be ſeen even in the dark.

I have alſo made with this Engine an Experiment which hath much ſurpriſed many, I fill a glaſs bottle about half full with water, and when the Air is very exactly exhauſted I ſhut it in the manner deſcribed in the concluſion of this ſection: afterwards I heat this bottle, but yet in ſuch a manner that that heat is ſcarce diſcerned by touch: I put it afterwards in water cooled with ice, and the cold makes this water boil in the bottle in the ſame manner as fire uſes to do. the reaſon of this experiment is that the ſmall heat which is given to the water is ſufficient to raiſe up vapours which make ſome preſſure in the bottle, but as ſoon as it is put into cold water the vapours condenſe and the preſſure ceaſeth: ſo that the water *in vacuo* is at liberty to boil up, and the heat, tho very ſmall, is able to cauſe it: which could not happen if the engine had not been very well exhauſted: for if any Air had

had remained in the bottle it would have made a preſſure and hinder'd the ebullition, even after the Condenſation of the Vapors. But the moſt certain proof of the exactneſs of this Engine is the Experiment which is made with Water purged of Air: I gave the manner of it in the Treatiſe of the *Nouvelles Experiences du Vuide*, printed about 12 Years ago, and I ſhall have yet occaſion to deſcribe it in this, *Sect. 3d. Exper. 6th.* therefore I ſhall ſay here nothing elſe of it, but that by that means I have found the Engine here deſcribed to be more exact than any I have ever ſeen; for with the others I could not make purged Water fall lower than ½ or half an Inch near the Level; which ſhewed, that there remained at leaſt the 384*th.* part of the Air in the Receiver; but with this Engine I have made purged Water fall ſo near the Level, that there was Reaſon to believe, that the ſmall heighth which appeared in the Pipe, came but from the narrowneſs of the Pipe, in compariſon of the Veſſel wherein it was ſunk; for it is known, that the Water remains higher in narrow Glaſſes, than in large ones.

This Engine being then ſo exact, that it leaves no Air able to make any ſenſible Effect, it is eaſie to judge, that the proportion betwixt the ſpecifick Gravity of Air, and that of Water may be found by it's means, without taking all the Trouble, to which one would be engaged in following the Method which Mr. *Bernoulli* hath very ingeniouſly given in the *Journal desſcavans* of the 31*th. July* 1684.

It is true that the Cautions he ſpeaks of are uſeful, when the *vacuum* is not made exact; but having an Engine ſuch as I have deſcribed, we need but to exhauſt very exactly ſome Bottle, whoſe Capacity is known, put it in a Scale with ſome weight on the other ſide, which counter poiſes it; and ſo letting the Air into the Bottle again, we need but to add ſo much weight in the other Side, as will counterpoiſe the Air got into the Bottle; and by comparing that weight with that which the Bottle containeth of Water, the required proportion is immediatly found. In the Book of Mr. *Boyle*, intituled *Exper. Phyſichomech. contin. 2d.* may be ſeen, that

the Proportion which hath been found by that manner, was as 1 to 834, and tho' the Engine was not yet at its greateſt Perfection, but left as I have ſaid about the 384*th.* part of the Air; this proportion did notwithſtanding agree pretty well with that which we have found ſince at the *Royal Society*, in making the Experiment, according to the way of Mr. *Bernoully*, with a Bottle that contained above 80 pounds of water, and which was weigh'd in Water, ſo that there wanted but about 1 ounce and a half on the other Scale, to counterpoiſe the Air of the ſaid Bottle: As this Experiment is in the Regiſters of the *R. S.* who will publiſh it when they think fit; I ſhall ſay nothing elſe of it, but that the difference of the Proportions found by theſe two Experiments, was ſo little, that it may very well be attributed only to the difference of heat, or moiſtneſs, or preſſure of the Atmoſphere, and not to the Fault of the Operation: So that it may very well ſerve for all the uſes it hath been hitherto applyed to. Nevertheleſs, as there might be ſome occaſions where it would be requiſite to weigh the Air with the greateſt exactneſs, I ſhall ſay that then it would be beſt to make no uſe of Scales, but only to counterpoiſe the Bottle in the Water. For by that means the Addition or Subſtraction of leſs than the hundredth part of a Grain might ſuffice to make a very ſenſible effect, making the Bottle fall to the Bottom, or riſe to the ſurface of the liquid, which is the moſt exact way of weighing that hath been yet found out, and which hath been publiſht in ſome of Mr. *Montanary*'s Works. Before I leave off this Subject, I cant forbear to ſay, that I have wondered, in reading the *Journal* quoted here above, and that of the 18*th.* of *June*, 1685, to ſee that Mr. *Bernoulli* ſhould have quoted Mr. *Boyl* to find fault with him, meerly upon the Report of others, for certainly if he had examined the thing himſelf, he would not have ſayd as he hath, in the *Journal* of the 31*th.* of *July*, 1684, that Mr. *Boyl* hath given no better means to weigh the Air, then in ſealing Hermetically Vials of Glaſs in the Furnace: Since I have here quoted a place of Mr. *Boyls*

Works

Works, where the contrary is clearly ſeen; he would not have ſaid neither in the *Journal* of the 18*th. June* 1685, that in the *Prolegomena* of the *Paradoxa Hydroſtatica*, Mr. *Boyle* ſeems not to diſapprove the way of weighing the Air in a Bladder; ſince in all that Preface, there is not a word of that Subject: It is in the 2 *d.* Experiment of the *Exper. Phyſich-mech*: That Mr. *Boyle* ſpeaks of weighing the Air in a Bladder, and he condemneth that Method, and ſaith that it is by means of an *Æolipile* that he hath found the Proportion to be relyed upon; methinks a greater precaution ſhould be made uſe of, before one doth undertake to cenſure the works of a Perſon of the Reputation of Mr. *Boyle*. I may ſay moreover, that after having made an enumeration of ſome things that were not quite perfected, in the Method of Mr. *Boyle*, ſince many Readers want diſcretion to diſcern it themſelves; it had been well to give them Notice, that there is no doubt but Mr *Boyle* knew very well all theſe things, but that in thoſe Beginnings where there had been ſo many Subjects that had not been workt upon yet with the *Air Pump*, he had not thought fit to ſtay too long upon every one; but judged what he had done to diſcover the ſpecifick Gravity of the Air, ſufficient for the need there was of it then.

Mr. *Bernoulli* hath ſo much ground to hope to augment the Number of great men, that I have beleived he could not take it ill if I made theſe ſhort reflections for the intereſt of their Honour.

Having ſayd what I have done to endeavour to render theſe Engines more exact, I ſhall add alſo how I have attempted to render the uſe of them more eaſie: the firſt thing hath been to grind the plate G G ſo exact that the receivers may be apply'd upon it and ſhut exactly without any need of the cement I was wont to put to the firſt engines; which gave alwayes trouble to be fitted and did ſtick ſo as to make both the Glaſſes and fingers greaſy: I have alſo perceived ſome fault in the manner which I had at firſt to ſtirr the things which were ſhut up *in Vacuo* I did make uſe then of

a wyre tied in an *Eelle Skin* which was apt to ſtink in a little time, and beſides I could move but one thing at once : I have then found that the Method which follows is much better. I take a receiver open at both end's as A A (*vide Fig.* 14) I apply upon its ſmall apertur e a plate of metal B B : This plate is pierced with many ſmall holes fit to admit the Braſs Wires I intend to make uſe of : I apply a wett leather upon that place and tye it about the aperture of the receiver, ſo that it is very well ſtopt with it : afterwards, I pierce the leather through the hole I have need of, and thruſt in the Braſs Wire C C which ſtops it very exactly : This Wyre being part within and part without the Receiver ; I have the conveniency to move what I pleaſe *in vacuo* without letting in any Air, and that Leather ſo applyed is not ſubject to ſtink, as the *Eel*-skin was ; I have yet the advantage, that I can in that manner move many things at once, if occaſion requires it : for there being many holes, I may paſs thro' them many Wires, one of which ſerveth to move one thing, and the other another ; and ſtill without letting any Air get in, becauſe theſe Wires make their holes very exact in the Leather, and fill them up exactly ; nevertheleſs for greater ſecurity, the Ring D D may be cemented upon the Receiver A A : and this Ring being fill'd with Water, it will be certain that no external Air at all can get in by the upper part of the Receiver : This Ring may alſo be fill'd with *Molaſſos*, or ſuch other liquid one may have a mind to introduce into the Receiver exhauſted of Air : For opening a little one of the holes of the Plate B B, the liquid runs very faſt into the *Vacuum* : I make uſe of this when I have occaſion to keep Glaſſes exhauſted of Air, and free from moiſture, I put in the Receiver A A the Glaſs I intend to keep with a cover very exactly ground to it, and as ſoon as the Air is well exhauſted, I apply the Cover upon the apperture of the Glaſs, and preſs it hard upon it, by means of the Wire C C : and having fill'd the Ring D D with *Molaſſos*, I open one of the other holes of the plate

B B,

BB, ſo that the *Molaſſos* enter into the *vacuum*, and cover the Glaſs incloſed in it, ſo that when the external Air comes in afterwards, there is not the leaſt danger it ſhould get into the ſaid Glaſs, and ſo the things that are ſhut in it may be preſerved as long as it is deſired, both from the Air, and from moiſture. I made uſe formerly of Turpentine, to hinder the outward Air from entring into the Glaſſes which I intended to keep ſo exhauſted for a long time; but ſince I have found it better to make uſe of *Molaſſos*: becauſe it is not apt to give a bad ſmell and taſt to the things that are preſerved.

New Uſes of theſe ENGINES.

IT is eaſie to Judge that Air having ſuch a ſtrength as it hath, both by its weight and by its ſpringineſs, and yet being inviſible; the Engins which ſerve to take it away and give it again at pleaſure, may produce ſuch ſurpriſing effects, that moſt people would take them to be ſupernatural: For example, once when I had time to ſpare, I made up a Braſs Veſſel upon which was applyed a large Glaſs Pipe, in which nothing was to be ſeen, but as ſoon as I ſaid the word, that it be fill'd with Water, then Water came into it, and remained in it as long as it was deſired; if afterwards I bad the Water to diſappear, and to go and change it ſelf into

to Wine, it was immediatly seen to go into the Brass Vessel again, and soon after a Wine of a fair Colour was seen to come out of it, which remained also in the Glass, as long as it was thought fit: But if I bad it to disapear, and to go and change it self into some other Liquor, For Example into Ink, the Liquor was seen as before to obey immediately; and what was more surprising, was that this Brass Vessel was suspended in the Air by a very small thred in a transparent Glass: so that it was seen clearly that no body touched neither the Glass nor the thred wherewith it was suspended: It was therefore certain also that there were no pipes through which the Liquors that were seen to appear in it might be conveyed to it: So that many people would not have scrupled to swear that these different effects must need be produced by some Supernatural Cause: Nevertheless it was nothing else but a Man which played an Air Pump in a hidden place, by whose means he rarefied or condensed again the Air in the great Glass wherein the Brass Vessel was suspended, and this Vessel was furnished within with Pipes and Valves, which according to the differing degrees of rarefaction of the Air, did convey into the Glass Pipe, sometimes one Liquor and sometimes another. It is then, I say, easie to Judge that abundance of Paper might be spent, if one would set down all the uses of this Nature to which this Engine might be applyed, but I fear to tire the Patience of my Readers, and I shall put here but what may be of a real use, or give some knowledge of Nature.

I might also reckon amongst the uses of this Engine the strength it can afford to produce great effects without the incumbrance of great weights: For a tube very even and well workt may be made very light and yet being emptyed of Air, it will endure the pressure of the Atmosphere: Nevertheless a plug very exact at one end of that tube would be pressed towards the other with a very great strength, at least if the tube was of a pretty great Diameter: for example if it was a foot Diameter the plug would be press't with the strength

ſtrength of about 1800 pounds. The Renouned Mr. *Guerike* hath the firſt tryed to apply that ſtrength to ſhoot leaden bullet's through a gun, as may be ſeen in the deſcription he hath given of it in his book of the Pneumatick engine: I have alſo endeavoured ſince to add ſome thing to his invention, as may be ſeen in the *Philoſophicall Tranſactions* for the mounth of *January* 1686: ſince that I have calculated that a bullet of lead of an Inche Diameter being thus ſhot through a barrel 4 foot Long ſhould acquire the ſwiftneſs to fly about 128 feet in a ſecond; but if the ſame ſwiftneſs was to be given to a bullet of a foot diameter it ſhould be made of Iron hollow within, ſo that it ſhould weigh but about 37 pound and half: For if it was made ſolid of Lead, it would weigh about 450 pounds, and ſo in paſſing the length of about four foot in the barrel, it would acquire but the ſwiftneſs of 32 feet in a ſecond: If the lengths of the barrels were encreaſed: So as for Example, to make the bullets to paſs 16 feet in them, they would acquire double the ſwiftneſs of what they did in but four feet: So that it will be found that a bullet 1 foot Diameter, which would weigh but 37 pounds and a half, might in a barrel about 17 feet long acquire the ſwiftneſs to fly 256 feet in a ſecond, which is very conſiderable: But it is to be obſerved, that when people will come to the tryal of it; there will be much to abate. 1. The friction doth hinder much more in a ſtiff Pipe than in the free Air. 2*dly*. The end of the barrel through which the bullet paſſeth muſt be ſtop't with ſomething ſtrong enough to uphold the preſſure of the Atmoſphere, and that being met in the way of the bullet, takes away alſo ſome of its ſtrength. 3*dly*. The bullet can't be ſo exact to the barrel, as not to let paſs ſome Air betwixt it and the ſides of the barrel, and ſo the *vacuum* being not perfect before the bullet, it is not driven with ſo great a ſtrength. 4*ly*. If even the *vacuum* ſhould remain perfect before the bullet, there would be yet much to be abated of my calculation: Becauſe I have ſuppoſed the ſwiftneſs of the Air as infinite, in compariſon of that which

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would be communicated to the Lead driven by it; but it is not ſo: For I may very well demonſtrate that the ſwiftneſs of the Air entring into the *vacuum* is to paſs but about 1305 feet in a ſecond at the moſt: So that when a bullet in a barrel hath acquired the ſwiftneſs to paſs but 100 feet in a ſecond, the Action of the Air upon that bullet will be diminiſhed a 13*th.* part or thereabouts. The R.S. hath of late cauſed ſome Experiments to be made to determine what ſwiftneſs the lead will effectualy acquire in the ſeverall length's of Barrells, but I ſhall not relate them hear: becauſe I doubt not but thoſe Gentlemen will continue them and publiſh them when they think it fit; and I think it enough to have ſaid thus much that it may not be pretended to draw from this invention much greater effects than it can produce: So I proceed to relate the Experiments I have newly tryed.

EXPERIMENT. 1*ſt.*

Since the Air Pump hath been found out it hath been very much endeavoured to make uſe of it, to preſerve ſuch matters as are ſubject to corrupt in the Air: The Illuſtrious Mr. *Boyl* hath ſeveral times cauſed eſſays of it to be printed and hath advanced the thing very much: The Famous Mr. *Guerike* hath alſo ſpoken of it in his Book: There were alſo ſome Experiments of it in the little Treatiſe I cauſed to be printed at *Paris* twelve Years ago under the Auſpices of the Illuſtrious Mr. *Huygens*; but all theſe Experiments have ſhewed that it is not enough to keep the bodies from the communication of the Air, which corrupts them, for they have alſo in themſelves ſome differing principles, which acting the one upon the other cauſe a fermentation, and ſo change the qualities of the body that was intended to be kept without alteration: And generally by that fermentation ſuch quantity of Air is produced, that

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the Veſſel can't contain it, tho' it be very certain that nothing is come in from without: if to remedy that inconveniency one would put the matters in ſo ſmall a quantity, that they could not ferment, they would looſe their taſt; if ſome Air is left incloſed in the ſame Glaſs, they grow muſty, or ferment; if they are boiled in the Digeſter, that ſtops the fermentation for ſome weeks, as I have publiſht in my firſt Treatiſe of the Digeſter, but afterwards the fermentation begins again, and if ſuch ſtrong boiling be reiterated, it changeth extreamly the Qualities of the Body: I believe then that it will be very difficult to find out the means to preſerve food; and eſpecially the Fruits in their conſiſtence and natural taſt: but if all that might be wiſhed for is not found in that, it will ſtill be much to draw nigh it, and to be able to preſerve all the ſubſtance of the Fruit, without any other alteration than that which a very light boiling may bring to it, and without being obliged to lay out any money to buy Sugar, or any other Ingredient fit to preſerve them: here is then what I have done upon that Subject.

The 26*th* of *June* I did ſhut up into two Glaſſes *in vacuo* ſome juice of Cherries; it became very clear in a days time the ſediment being fallen to the bottom. The 30*th*. of *June* I perceived that both the Glaſſes began to produce ſome Air, I did put one of them to boil before the third part of the Air needfull to fill it up again had been produced, ſo that it boiled with a very little heat. I left the other Glaſs to continue to ferment, ſo that the 4*th*. of *July* it was quite filled up again: I did ſet it in a Preſs to keep it ſhut, but the Air that was produced was in ſuch a quantity, that it burſted out notwithſtanding all the care I could take; whereas in the Glaſs which I had boiled, the fermentation was quite ceaſed: ſo that it appeared ſtill in the ſame ſtate above four Months time: But the 17*th*. of *November* I carried both Glaſſes to *Greſham* Colledge, and on the Morrow the *Royal Society* found that the fermented Glaſs had been ſettled ſooner than the other, which appeared yet thick by reaſon of the Moti-

on of the preceding day; but the fermented Liquor had the taſt but of a ſmall Wine green and rough; whereas that which had been boiled *in vacuo* had a taſt of Cherries very pleaſing and Natural.

EXPERIMENT 2*d.*

The preceding Experiment having ſhew'n me that the boiling juice of Cherries *in vacuo* did hinder it from fermenting; I would ſee if the ſame thing would happen to other juices: I took then the 6*th.* of *December* a good quantity of juice of Apples boil'd in the Digeſter with ſome ſmall Beer and a little Sugar: I ſhut it *in vacuo* with a Gage, but it happened that in exhauſting the Glaſs, a Froth roſe up to the cover which obliged me to leave about a 6*th.* part of the Air in the Glaſs; having taken it away from the Engine I did put it to boil, and afterwards left it to ſettle: I ſaw that for 15 days time it did clarify by little and little; but afterwards it became thick again: Nevertheleſs I was ſure by means of the gage ſhut up in the Glaſs, that there was no Air produced within: The thing ſtayed in that ſtate till the 13*th.* of *Aprill* of the following Year, and then I opened the Glaſs, and found that the Juice had yet the ſame taſt as it had when I had ſhut it up.

Tho' the ſucceſs of this Experiment was differing from the preceding in ſome circumſtances whoſe cauſes, it may be, will be diſcovered in time: at leaſt the fermentation was found to be ſtay'd in the one and the other of theſe Juices, which was what I chiefly did look for: So that I had great hope, that if the whole Fruits were heated in *vacuo* they might be preſerved as well as the Juices: I have then begun this Year 1686 to make ſome Experiments about this Subject, they have pretty well ſucceeded, as may be ſeen in the *Journal* which I have kept, and of which I ſhall give here ſome abſtract.

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EXPERIMENT 3*d*.

The 29*th*. of *May* I took ſome green *Gooſberries* boiled in the Digeſter: I put them with their Juice in a Glaſs fill'd about 2 thirds with them, and ſo I ſhut them in *vacuo* with a gage; on the morrow I ſaw this Fruit had given but little Air; nevertheleſs I did put the Glaſs in Water and heated it till it began to boill, and immediatly I took away the fire: Since that time the gage doth not at all alter, and there is no Air produced in the receiver. I had at the ſame time ſhut up raw *Gooſberries* with Sugar and *Cinnamon* in an other Glaſs exhauſted of Air; but no longer time then the Morrow I found there was already Air enough produced to fill up the third part of the Glaſs, which I took for a token that the Fruit began already to ferment: So that I put it alſo in the ſame *Balneum Mariæ*, and heated it in it till the *Gooſberries* appeared boil'd and wrinkled, and till the Sugar were diſſolved by the Juice of the Fruit; that did hinder the fermentation for ſome days, but the 14*th*. day the Glaſs was found full of Air, and the Fruit which was ſhut in it had a very pleaſing taſt: This Experiment made me ſee that there is more difficulty to preſerve Fruit raw than boil'd.

EXPERIMENT. 4th.

Having ſeen in the preceding Experiment, that Fruit raw ſhut up *in vacuo* produceth immediatly a great quantity of Air, if it is not heated, I would ſee if I could hinder it by putting the Fruit to heat as ſoon as it would be *in vacuo*: So that the 31*th*. of *May* I ſhut up *in vacuo* new raw *Gooſ-*

berries with Sugar and Cinnamon, in the ſame manner as the preceding, and put them to heat immediately after. I ſaw that in the time they were upon the Fire they did alſo produce Air enough to fill about a third part of the *vacuum* of their receiver, but ſince that they were a pretty while without producing any: Nevertheleſs a fourth night after they had fill'd the two thirds of their Glaſs with Air; but ſince that time I do not ſee that the Air encreaſeth, and the *Goosberries* ſeem to keep well.

That made me ſee that it is a great matter to heat raw Fruit as ſoon as it is ſhut up.

EXPERIMENT. 5*th.*

The raw Fruit of the preceding Experiments having been mingled with Sugar, which it were to be wiſh't wee might excuſe, as well to avoid laying out Money for Sugar, as altering the taſt of the Fruit: I would alſo make the ſame Experiment without addition of Sugar. So that the 6*th.* of *June* I did ſhut up green *Goosberries in vacuo* and put them to heat as ſoon as I had taken them out of the Engine; they gave a very thick juice being almoſt all burſt, but they did not wrinkle as do moſt of thoſe which are put with Sugar. Five weeks afer I opened the Glaſs, and I found the *Goosberries* with as good a taſt as *Goosberries* a little boil'd can be.

The 12*th.* of *June* I reiterated the ſame Experiment; and I heated the *balneum Mariæ* where I had put the Glaſs till the Water boiled: Notwithſtanding there appeared but as much Air in the Glaſs as was needful to fill about a fourth part of its *vacuum*: I have kept it ever ſince, and I do not perceive the leaſt change, neither to the colour of the Fruit, nor to the quantity of the Air, and all is yet in the ſame ſtate as it was the next day after it had been ſhut up.

This Experiment ſhews that Sugar would rather hinder than

than *Facilitate* the preſervation of Fruits after this new manner: Nevertheleſs if it was deſired to preſerve ſome to appear freſh and entire, Sugar ſhould be put, as will be ſeen in the 19*th.* Experiment: It may be obſerved here alſo, that the Juice of *Goosberries* without Sugar is much thicker than that of *Goosberries* with Sugar, tho it ſeems that a hard body as Sugar is, ſhould rather be fit to take away Liquidity than to give it.

EXPERIMENT 6th.

Having ſeen by the third Experiment, that Fruits much boiled in the Digeſter did not at all ferment when they were ſhut up and heated *in vacuo*, I would try whether it was not that ſtrong boiling they had ſuffered which hindred them from fermenting: So that the 30*th.* of *May* I took ſome Juice which remained of the *Goosberries* I ſpake of *Sec.* 2*d. Exp.* 3*d.* I did put half a pound of it in a Glaſs that was juſt fill'd up with it, and did ſhut it with a ſcrew. The 9*th.* of *June* this Juice did ferment ſo ſtrongly, that I believed it would have all got out of the Glaſs, notwithſtanding the Scrue which preſſed the cover: I took it away then and found its taſt pleaſing but apt to fly in the head if taken in quantity.

This Experiment confirmed me, that the heat which is applyed to matters ſhut *in vacuo* doth hinder them from fermenting; ſince this Juice did ferment ſo for want of having been heated as its Fruit had been.

EXPERIMNT 7*th.*

Becauſe one might have the ſuſpicion that the Glaſs of the preceding Experiment had fermented by reaſon it was fill'd up

up with juice; whereas the Glass of the 3*d.* Experiment had almost nothing but Fruit, I would make an Experiment quite contrary: The 5*th.* of *June* then I boiled green *Goosberries* with Sugar in the Digester: I did shut up their juice in a bottle *in vacuo* and made it boill upon the Fire, I did shut at the same time much of the Fruit with some Juice in an other Glass which was quite fill'd with it, and to which I fastened the cover with a screw without putting it *in vacuo*. The 9*th.* of *June* the Glass where was the Fruit did ferment very strongly: but the Glass where there was but Juice did not work at all. The 16*th.* of *June* I opened the Glass where the Fruit was and found it had much fermented and had a Wine tast: The Glass where the Juice was kept very well, but did not clarifie.

This Experiment together with the preceding, and the third shews that whether it be the Fruits or the Juices that be shut without exhausting the Air, they Ferment: and on the contrary, whether it be the Fruits or the Juices that be heated *in vacuo* they may be kept a long time in that state, without any alteration.

EXPERIMENT. 8*th.*

To confirm my self more that the heat which is given to Fruits shut *in vacuo*, doth hinder their Fermentation, I would try it upon Fruit which had already begun to ferment, so that the 12*th.* of *June*, I took Goos-berries which had been still'd two days past in the instrument represented *Fig.* 11. they had already a fermented tast, and would soon have putrified: I did shut them up *in vacuo*, and put them immediately in a *Balneum Mariæ*, which I heated till the Water boiled; I kept the heat above and hours time, during all which time I perceived not that any more then the 6*th.* part of the Air needful to fill up the *vacuum* in the Glass had been pro-

produced, and ſince that time the Mercury in the Gage remains ſtill to the ſame heighth, and ſo I do not perceive the leaſt change to it no more than to the Colour of the Fruit.

This Experiment confirmed me very much in the hope of the good Effects that are to be expected of this manner of preſerving Fruits.

EXPERIMENT. *9th.*

Having ſo well ſucceeded to hinder fermentation in matters ſoft and moiſt, I believed I would ſucceed yet eaſier in green peaſe, which are of a harder and dryer conſiſtence, but I was extreamly miſtaken, as may be ſeen by what follows. The *19th.* of *June* I ſhut up *in vacuo* green Peaſe, which were a little begun to be boiled; they did not fill half their Glaſs, and as ſoon as they were taken out of the Air Pump, I did put them in a *Balneum Mariæ*, which I heated till it began to boil.

The *20th.* of *June* tho' I had taken ſo much care in ſhutting the Peaſe, that they might not fill up their Receiver too ſoon, I ſaw they had almoſt filled it quite full already, ſo that I put them again in the *Balneum Mariæ*, where they produced a great quantity of Air, which lifted up the Cover, and went out of the Receiver, but the Cover ſhutting again as ſoon as ſome Air was got out, did hinder the external Air from getting in; and when it was cool'd again, the cover was found yet ſtrongly preſt againſt the aperture of the Glaſs: And I ſaw by the Gage, that there remained but about a third part of the Preſſure which was in the Glaſs whilſt it was hot: ſince that time the Peaſe remained ſix days without producing any Air, but the *26th.* of *June* they began to produce ſome, and in two days time they had fill'd their Receiver: I reſolved then to ſee if the boiling of them in the Digeſter

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would have more power then what *I* had done before: encreaſed the heat to evaporate the drop of Water in ſix ſeconds; and having taken out the Peaſe, *I* put them in a ſmaller Receiver, that it might be the better filled: *I* exhauſted the Air from it, and put it again in a *Balneum Mariæ* whoſe Water *I* heated till it boiled: ſince that time the Peaſe ſeem to keep very well and produce no Air: but a great deal more Trouble hath been required to preſerve them ſo, than to preſerve Gooſe-berries.

EXPERIMENT 10*th.*

As I had obſerved that *Rasberries* keep in Gelly better than any other Fruit(*vid. Sec.* 1.*Ex.*25)I believed they might alſo be preſerved very eaſily being heated *in vacuo*: Nevertheleſs I was miſtaken in it as may be ſeen by the following Experiment. The 23*d.* of *june* I fill'd a Glaſs with raw *Rasberries*, and having ſhut it *in vacuo* I put it immediately in a *balneum Mariæ* which I heated ſo as to boill: The *Rasberries* kept very well their colour tho' they gave a pretty great quantity of a fine red juice, and they ſunk ſo that the Glaſs remained but half full. The 7*th.* of *july* I begun to perceive the *Rasberries* produced ſome Air: So that I put again the Glaſs in a *balneum Mariæ* whoſe Water I cauſed to boill: The 4*th.* of *Auguſt* I found that the *Rasberries* had finiſhed to fill up their receiver with Air, I took them out and they were found to have a good ſmell and taſt of *Raſberries*, eſpecially the juice that was come out of them had a taſt extreamly pleaſing.

This Experiment made me ſee that it is to be wiſhed to have ſeveral ways to preſerve Fruits; becauſe what is fit for ſome may be unfit for others, and thoſe which keep beſt in Gelly and produce no Air at all in it, are not hindred from producing ſome, by being twice boiled

ed *in vacuo*; whilſt other Fuits which produce a great quantity of Air, being ſhut in Gelly produce none at all being but once boiled *in vacuo*: There is no doubt but this kind of Experiments may furniſh fair occaſion to reaſon upon the Nature of Foods.

I ſhall not particulariſe here all the other Experiments of this Nature which I have made upon Fruits: I ſhall only give ſome Obſervations I have taken from them.

1. *Straw-berries* can't bear to be heated: And tho' I was certain that Glaſſes could not be better ſhut than thoſe in which I did heat them: I have always ſeen with ſurpriſe, that a ſmall heat did alter them extreamly.

2. *Cherries* which I have taken notice to be one of the Fruits which produce the leaſt Air when ſhut up with Gelly (*Vid. Sec.* 1. *Exp.* 27) are on the contrary very difficult to keep from producing ſome when ſhut *in vacuo*, and I have been obliged ſometimes to heat them three times together.

3. *Apples* which do not at all wrinkle being ſhut up in Gelly (*Vid. Sec.* 1. *Exp.* 37.) do on the contrary wrinkle extreamly when boil'd with Sugar *in vacuo*, and ſhrink almoſt quite away; but the Liquor from them hath a very pleaſing taſt.

4. *Currants* do produce a pretty great quantity of Air in the time they are heated *in vacuo*, but afterwards by ſucceſs of time this Air vaniſheth away, and I have kept above four Months a Glaſs where ſome *Currants* were ſhut up with a gage, and the Mercury was as low as it could go, tho' in the time I heated the Glaſs the Mercury was riſen in the gage to the higth of above 15 Inches; at laſt the Glaſs was opened in the R. S. the 17*th*. of *November* and the Fruit was found very good.

EXPERIMENT. 11*th.*

I have alſo made ſome Experiments to ſee if this manner of preſerving bodies would have effect upon Meat as well as upon Fruits. The 17*th.* of *February* I ſhut up Gelly of Meat moderately ſeaſoned with Salt in a Glaſs which I exhauſted of Air, and heated it afterwards till it could hardly be held with the hand: I put at the ſame time ſome of the ſame Gelly in another Glaſs, from whence I took out no Air, I melted it and heated it very well, and then covered the Glaſs. The third day the Gelly in the Glaſs with Air appeared to produce ſome bubbles, and the fifth day having taken it out I found it ſtunk very much: Nevertheleſs the Gelly *in vacuo* appeared not changed nor produced any Air. The 16*th.* of *February* me thought the Gelly *in vacuo* began to produce ſome Air: So that I did put again the Glaſs in hot Water and heated it about as much as the firſt time, and this heat produced Air enough to raiſe the Mercury almoſt to the top of the gage, but the cover of the Glaſs was not lifted up, and afterwards the Mercury ſunck down by little and little for the ſpace of 3 or 4 days. The 18*th.* of *March* I had ſome new Suſpicion that the Gelly produced ſome Air, but ſcarce to be diſcerned: So that I opened the Glaſs and found that the Gelly had not the leaſt beginning of corruption: I melted it and put it into another Veſſel where it congealed very well, and kept ſweet the ſpace of two days in the open Air.

EXPERIMENT. 12*th.*

The preceding Experiment having ſhew'd me that it would be eaſie to preſerve Gellies for the ſick as long as would

would be neceſſary even in the longeſt Voiages; being but a ſmall trouble to heat them from time to time: I would ſee if raw Meat would alſo be preſerved: So that the 20*th.* of *March* I put ſome raw Beef and a little Salt and Water in a Glaſs, which I exhauſted of Air, and put it immediately after in a *balneum Mariæ*, which I did not heat quite enough to make it boil, but the Water ſhut with the Meat boil'd apace: The 24*th*, of *March* ſeeing the Meat had produced about a 14*th.* part of the Air needful to fill up the *vacuum* of the receiver wherein it was ſhut, I put it again to boil, which made the Mercury riſe in the gage about four times higher than it had done before: So that there was about a third part of the Air that had been needful to fill up the Glaſs; but on the morrow I found the Mercury almoſt quite ſunck again: The *6th.* of *Aprill* ſeeing the Mercury in the gage began again to riſe, I put again the Glaſs to heat which cauſed again the Mercury to riſe much, and on the morrow I found it much fallen, but not quite ſo low as before I heated it: This time the Water in the Glaſs boil'd not, tho' I heated the *balneum Mariæ* wherein it was till it boil'd. The 28*th.* of *Aprill* ſeeing the Mercury began again to riſe in the gage, I put again the Glaſs into the *balneum Mariæ* which I heated till it boil'd, and the ſucceſs was as at the other times. The 15*th.* of *May* I perceived that the Meat had produced a great quantity of Air ſince two days: So that I took it out and found it to have ſome bad ſmell, I hung it and after it had been ſome hours expoſed to the Air there remained almoſt nothing of the ſmell, and I believe that if I had taken it out or put to heat, two or three days ſooner there had been no tendency to Corruption.

This Experiment ſhewed me that it would be more difficult to peſerve meat raw than dreſs't; but yet with ſome care, raw might be kept alſo for a conſiderable time.

EXPERIMENT 13*th.*

The 4*th.* of *July* I did ſhut up *in vacuo* ſome Gelly well clarified, ſeaſoned with Sugar, Wine, *Lemon* juice, and *Cinamon* Eſſence: As this Gelly was ſoft and very little evaporated, it could not have kept above two or three days in the open Air; nevertheleſs having been heated *in vacuo* I kept it a whole Month in the hotteſt time of the Year, and found not the leaſt alteration in it: it had ſtill its taſt and ſmell, as pleaſing as before it had been ſhut up.

This Experiment may be uſeful, becauſe the time to be ſpent in making Gellyes makes them much dearer, but having the conveniency to preſerve them without any danger of Corruption, a great quantity may be made all at once, and ſo the Time imployed in it will be of little Moment.

EXPERIMENT 14*th.*

The 13*th.* of *June* I ſhut up a raw Mackerel cut into peices in a Receiver, which was about half fill'd with it: I exhauſted the Air and put it immediately in a *balneum Mariæ*, which I made boil ſoftly for almoſt a quarter of an hour: All this produced about a third part of the Air that had been needful to fill up the *vacuum* in the Receiver. The 17*th.* of *June* ſeeing the Mercvry roſe conſiderably in the gage that was ſhut in with the Mackrel, I put it again in the *balneum Mariæ* which I made boil again: And after it was cold, the Air produced in the Receiver was about half of what had been needful to fill up the *vacuum*. The 21*th.* of *June* I opened the Glaſs and found that the Mackrel was not

not at all corupted, but had as good a taſt as if it had been taken that day.

That is all that my leiſure hath permitted me to do to this time, as well upon Fleſh as upon Fiſh, and methinks that is ſufficient to give reaſon to hope that if this kind of Food is well dreſſ't, and afterwards ſhut up *in vacuo*, the way how to keep it may be eaſily found out: it might alſo be tryed if it will Facilitate the thing to ſhut the Meat in one Glaſs and the Moiſture that comes from it in the other: But I ſee but little reaſon to fear it will not ſucceed, eſpecially if the Fiſh or Fleſh are dryed a little firſt, in the ſame manner as was the Fiſh I have ſpoken of in my firſt Treatiſe of the Digeſter *chap.* 3. *Exp.* 9. for ſhutting them ſo *in vacuo* there will be no fear that they dry nor diminiſh any more.

EXPERIMENT 15*th.*

The Experiments I had made upon Malt in the Digeſter (*Sec.* 1. *Exp.* 9.) having taught me that provided the tincture of the Malt be extracted, the leaſt heat that can be given is the beſt; becauſe the Drink is the leſs rough and ferments better: I believed the Air Pump would be fit for that uſe; becauſe it opens extreamly the Bodys the tincture is to be extracted from, and the Air getting out from all their Pores, the Liquor Penetrates them much eaſier: I did then make this Experiment the 5*th.* of *July*: I put 5 ounces of Malt with 25 ounces of Water in a Glaſs which I exhauſted of Air, and which I put afterwards in a *balneum Mariæ*, whoſe Water I heated till the fingers could not endure it, but not to boil; it remained in it about a quarter of an hour. The 9*th.* of *July* I found that the Glaſs was fill'd with Air produced by the Malt: I took out the Wort and put it with ſome Beer yeaſt in a Glaſs whoſe cover I preſſed not, it began immediately to ferment ſtronger than any I had yet

ſeen

ſeen: So that about 24 hours after, it was ready to be bottl'd: I kept it in bottles about three weeks and then found it clear and good: Ibelieve notwithſtanding that it would yet be better to boil the Wort ſome time with Hops; But all my deſign in this Experiment was to ſee if I could not extract the tincture of Malt with a very little heat, and that ſhew'd me enough to make me certain that this way of making Drink may become uſeful in time.

EXPERIMENT *16th.*

I have had thought alſo that this Engine might contribute much to make very good Drinks, yet an other way than that I have ſpoken of: For it is known that all the compounds that are made are much the better when all their materials are perfectly mingled: So that I believed that having ingredients as good as thoſe that are in the Wine, ſomething as good as Wine might be made, provided they may be well incorporated together: Yet I confeſs that there is but little hope to make incorporations as perfect as thoſe Nature doth, but we may at leaſt endeavour to draw as nigh to it as we can: And as matters ſhut up *in vacuo* may be heated and agitated as much as one pleaſeth without any danger of any evaporation, there is reaſon to believe that by that means matters may be incorporated together much better than when, as in other caſes, the moſt ſubtile parts are ordinarily loſt. To aſſure my ſelf the better of it the 30*th.* of *June* I took Sugar diſſolved with equal weight of Water: and put twelve parts of it upon four of *Lemon* Juice, one of Spirit of Wine rectified, and ſix of common Spring Water: I ſhut all up in a *Florence* Flask which was about half full with it and having exhauſted the greateſt part of the Air I put it to boil upon the Fire: I kept it afterwards ſix days without opening it, and then I found the Liquor very

very good and pleaſing, and to be prefered to that made only by ſimple mingling.

The chief advantage which may be drawn of this Experiment, is that the proportion may be altered as is thought fit, whereas natural wine muſt be taken ſuch as it is given us by Nature: I prefere alſo the boiling *in vacuo* to that in the Digeſter for matters that are already fit for our ſtomacks and have no need to be cook't: becauſe the boiling in *vauuo* doth ſcarce alter them; whereas the boiling in the Digeſter is able in a little time to take away from the Gelly its congealing quality: ſo that tho the Digeſter might preſerve the ſpirits as well as the *vacuum*, I Eſteem the *vacuum* to be prefered in this occaſion: and there is no need to make it very perfect: for provided the heat makes not the preſſure in the glaſs as ſtrong as that which is without, one may be ſure nothing can get away.

EXPERIMENT 28th.

The ſame reaſon which perſuaded me the *vacuum* Would be uſefull to extract the tincture of the Malt, made me believe alſo it would help much in making of ſweet-meats: for as the whole is to make ſirup ſoak thoroughly into the fruit and alter it the leaſt that can be: It is plain that the reaſons given Experiment the 15*th.* muſt alſo have place in this occaſion: I tryed it the 15*th.* of *June*: I took orange peeles pared very thin and put them to ſoak in cold water for four and twenty hours: they became very ſoft and gave a great deal of bitterneſs to the water: I put them afterwards *in vacuo* with new freſh Water, and having let the Air in again, they all ſunck under Water: I exhauſted their Glaſs again, and then the peels produced a great quantity of bubbles and began again to ſwim, I let in the Air: and the peels ſunk again; having reiterated that ſeveral times, the Water

ter had so well extracted all the bitterness from the Peels that I did but boil them a little in Syrup, and afterwards put them with their Syrup in a Receiver which I exhaust of Air, and having exhausted and given the Air again several times as when they were in the Water, they became so well penetrated, that they all fell to the bottom of the Syrup, and so having put them to dry, I found them fair and transparent and of a very good tast: So that by means of our Engine they are easily deprived of their bitterness, and there is no need to boil them in three several Syrups as many Confectioners do.

EXPERIMENT. 18*th*.

The *6th*. of *July* I boil'd with Water in the Digester some *Lemon* Peels pared very thin, which I had kept 24 hours in cold water, to which they had given much bitterness: I encreased the heat till the drop of water began a little to boil I took away the peells whilst they were yet pretty hot. so that being exposed to the Air the moisture ran and evaporated away very soon: and in about a minuts time I put them in thick syrup a little hot and so shut them *in vacuo:* Yet I could not at first exhaust the Air exactly: Because the Syrup boiled too high and would have got out of the Glass; but in the mean time the heated Air got out of the peels, and when they were cooled I exhausted the Glass as exactly as may be: I left the peels to be purged of Air *in vacuo* about two hours: and afterwards let in the external Air to the end that by its presfure it should force the Syrup which was not yet cold into the Pores of the peels which were not cold neither: I left them so till the morrow, and then they were just ready to sinck under the Syrup, but since it was very thick they did not quite: I made them boil a little upon the Fire and afterwards took them out with a fork, and having let the Sy-

Syrup drain a little from them, I put them in a Stove where they dryed in a little time: And theſe Peels proved thicker and clearer and better penetrated than thoſe which are made in this Country; and leſs ſubject to be ſpoil'd than the *Citron* Peels that come from *Spain*, becauſe they were penetrated with a thicker Syrup.

EXPERIMENT 19*th.*

The 31*th.* of *May* I ſhut up *in vacuo* green *Goos-berries* raw with Sugar and Cinamon, and put them immediately to heat: in the time they were upon the Fire, they produced about the third part of the Air that had been needful to fill up their Receiver, but ſince that they were a long while without producing any: in a fortnight they had fill'd about two thirds of their Receiver with Air, but ſince that I have not perceived that the Air doth encreaſe, tho' the Fruit and Sugar be in ſuch a proportion to the Glaſs that they fill'd it up before it had been heated, and ſince by the heat the Juice of the Fruit hath diſſolved the Sugar and made a Syrup which fills up all the *interſtices*, the Glaſs is yet three-quarters full: There is then reaſon to believe that theſe *Goos-berries* producing ſo little Air do not at all alter: The Sugar diſſolved by the Juice of the Fruit is very clear and Tranſparent: There are ſome *Goos-berries* which tho' very wrinckled ſwim at the top, and there are others which are ſo well penetrated with the Sugar, that tho' they be very big and plump they ſink to the bottom: So that it is not to be feared they ſhould ſhrinck, when the Air will be let in agin into the Glaſs, and I make no doubt but in this manner a kind of ſweet meat may be made extraordinary fine and little Sugar'd.

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EXPERIMENT 20*th.*

The 10*th.* of *June* I boiled green *Goos-berries*, *Lemon* pulpe, a whole *Orange*, and ſome Sugar without Water in the Digeſter: I increaſed the heat to evaporate the drop of Water in 8 ſeconds: I obſerved not the proportion of Sugar I did put in, only I put it in at the ſame time with the Fruit to fill up the *interſtices*: The Syrup was very good; I included ſome of it with part of the Fruit in a Glaſs, and fill'd it to the top, then I ſhut it exactly, and made the cover firm with a ſcrew: 4 days after this Syrup began to ferment and get out of the Glaſs: So that I put a Porenger underneath to receive it, and ſaw that this Juice ſo received loſt its liquidity and became a very pleaſant Gelly which had a Wine taſt: Tho' this Experiment be not made by means of the Air Pump, I thought it would not be improper to inſert it in this place; becauſe it may be a patern for Confectioners to make eaſily fine Gellys, and of an extraordinary taſt: And moreover this effect of fermentation to have ſeparated a Subſtance fit to congeal, deſerves, methinks, not to be omitted.

EXPERIMENT 21*th.*

Sweet Meats requiring Sugar well purified to be fair and good, I have thought alſo of ſome ways to clarify Sugar more eaſily than it is generally done. I believed at firſt it would ſuffice to diſſolve Sugar in about an equal quantity of Water, and to keep this diſſolution *in vacuo*, till it was ſettled and became clear and tranſparent: and to hinder the fermentation which happens very ſoon to this kind of Diſſolutions, I thought I needed but boil them *in vacuo*, I did therefore execute

cute the thing after the aforeſaid manner, and *I* have kept ſuch Diſſolutions for many Months together without any fermentation; but yet they did never become very clear. *I* believed then it was needful to add ſomething to precipitate the Molaſſos: ſo that the 18*th*. of *june* *I* diſſolved 6 ounces of 5 penny Sugar in 8 ounces of Water, and having divided this Diſſolution in 3 equal parts, *I* powred them into three Vials, in the firſt there was Pipe Clay in Powder; in the ſecond Glaſs powdered, and in the third nothing. *I* exhauſted the Air out of theſe three Glaſſes, and put them together in the ſame *Balneum Mariæ*, whoſe Water *I* heated till it began to boil; *I* took immediatly the Vials out, and put them to cool: Since that *I* obſerved that the diſſolution where the Pipe Clay was, became much clearer and tranſparent than both the others; and on the contrary that wherein the powdered Glaſs was, remained browner and thicker than the others; which ſhews that tho' many Sugar Bakers believe, that their Clay ſerves only to hinder their Sugar from drying too ſoon, it hath yet another uſe: and in the ſame manner that Glaſs is fit to precipitate Salt, ſo Pipe Clay is fit to precipitate Molaſſos: I do therefore believe if People would for this Experiment make uſe of great Glaſſes big enough to hold ſeveral Gallons, great quantities of Sugar might be refined with leſs trouble and quicker than after the ordinary way; for here we ſhould but keep the Diſſolution till it were ſettled, and having drawn it clear, ſet it to evaporate; the diſſolution which would remain upon the Clay being in little quantity might bee ſoon filtrated: ſeeing eſpecially that the diſſolution ſo mingled with Clay do filtrate much quicker, than when no Clay hath been put in it: The Molaſſos which remain amongſt the Clay, might be put to ferment, and give their ſpirit as is generally done. At leaſt by this means, it will be eaſie to have always Sugar very clear, ready diſſolved, to employ in ſeveral Uſes,

as to make *Limonades*, *Punch*, *Gellies*, &c. Or elſe it may be evaporated to make Sweet-meats.

EXPERIMENT. 22*th*.

Having ſeen by the preceding Experiment, that it is ſo eaſie to preſerve Diſſolution of Sugar well clarified, I would ſee if one could not alſo preſerve ſeveral Juices of Fruits or Plants, which being well clarified, would not only be very convenient to ſeaſon *Gellies*, *Limonades*, *&c.* But might alſo be imployed in Phyſick, much more effectually than diſtil'd Waters, which are kept in the *Apothicaries* Shops. I had already ſeen *Sect.*2 *Exp.* 1. That the Juice of Cherries had clarified and kept very well: I would then alſo try if Lemon and Orange Juice would do the ſame. So that the 12*th*. of *January* I ſhut up the Juice of three *China* Oranges in a Glaſs which was about three quarters full with that, and I exhauſted the Air from it; I kept it ſo till the 1*ſt*. of *February*, but then ſeeing the Juice did ferment, and produced much Air, I took out by decant the clear Juice, and having ſhut it in a Vial *in vacuo*, I heated it almoſt to boil; it kept very clear and traſparent till the 13*th*. of *June*; but then methought it prov'd rediſh: So that I took it out, and found that the cover was almoſt looſen'd, the Juice had a very pleaſing taſt, but yet had ſomething of ferment.

This Experiment ſhewed me, that it would be eaſie to have always Juices of this kind clear and pure; for ſince this hath kept almoſt ſix Months without being touched: there is Reaſon to believe, that if it was heated but once in two or three Months it would keep above a Year.

EXPERIMENT. 23*th.*

The 28*th.* of *April* I had a good quantity of Juice of *Lemons*, which I had kept three days in the open Air, and which had turned for the moſt part into a *coagulum*: I did put it into two Glaſſes upon the *Air Pump*, but I could not exhauſt the Glaſſes very exactly, becauſe Juice producing a great quantity of Air, it roſe up all in a froth, and ran over the Brims: I did therefore ſhut them, when there remain'd yet about a 15*th.* part of the Air they contained before, and I found two days after, that they had already filled up their Receivers, tho' there was about a quarter leſs Juice in each Glaſs, than it could have contain'd. I let them continue to ferment, without opening them, and their cover did but a little lift up when the Air produced within was ſtrong enough to get away; nevertheleſs, eight days after, there was already one of thoſe Glaſſes which grew muſty; from whence it might be concluded, that the external Air had had ſome communication into it. I did therefore put the Glaſſes again upon the Engine, and having exhauſted, and ſhut them as the other time, I put them immediately in a *Balneum Mariæ*, and that failed not to ſtay their Fermentation. The 22*th.* of *May*, I found that one of the Glaſſes had yet produced ſome Air; but the other kept very well, and the 29*th.* of *June* when I had occaſion to make uſe of it, I found the Cover extreamly preſt againſt the Aperture of the Glaſs, and the Juice was very good, and clearer than it can be had by filtration; and the ſediment being mingled with Sugar, made a meat very delicate and pleaſing.

I was ſurpriſed at the difference found in this Experiment betwixt two Glaſſes fill'd with the Juice of the ſame Lemons; but having ſince found the ſame thing with Juice of Oranges as will be ſeen in the following Experiment: I have believed

ved, the cauſe of this difference came, becauſe in extracting the Juice, the Fruit was firſt preſt with the hands, and afterwards the peels put in a Preſs, which was able to ſqueeze out ſome Juice from other parts, than Acid; and ſo the Glaſs where was the juice drawn by that ſtrong preſſure, was the fitteſt to ferment, as I have obſerved *Sect. 1ſt. Exp.* 35 that the Acid of the Lemons and Oranges is not ſo fit to produce Air, as the other parts: Mean while the Fermentation of theſe kinds of juices doth not impair their taſt, it is but muſtineſs that ſpoils them, and this may be ſurely prevented by keeping them *in vacuo*.

EXPERIMENT 24*th*.

The *6th*. of *May* I ſhut up *in vacuo* ſome juice of Oranges which I had kept two days, it was in two Glaſſes, and began already to ferment; ſo that I could not make an exact *vacuum*: becauſe the Liquor roſe in froath and ſpilt. I was therefore obliged to leave in them about a fifth part of the Air they contained before: I put them upon the Fire in a *Balneum Mariæ* as ſoon as the Air was exhauſted, and that did not fail alſo to ſtop the Fermentation. The *3d*. of *July* one of the Glaſſes was very well clarified, and having drawn the clear juice from the ſediment, I found it of a very good taſt, and it had produced no Air: the other Glaſs was not clear yet, ſo that I kept it till the *10th*. of *Auguſt*; but then ſeeing it did not clarify, and that it had produced Air enough to fill up its Glaſs, I took out the juice, and found it to have a very good taſt.

Theſe Experiments confirmed me, that to heat the Matters *in vacuo*, is very fit to hinder their Fermentation; and that by that means, there is reaſon to believe we may always have freſh juices, and better clarified than they could be by filtration.

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EXPERIMENT. 25th.

I have alſo try'd if I could caſt Metals *in vacuo*; becauſe very often it happens, that the included Air keeps the melted Metal from filling the Moulds exactly: I thought I might remedy this inconveniency by extracting all the Air: I did moreover imagine, that the melted Metal having fill'd up a mould exhauſted of Air; and being preſently preſt upon by the whole weight of the Atmoſphere, it ſhould be driven into the ſmalleſt ſtroaks printed in the Mould, and ſo would take a more exact impreſſion than uſually it doth. I did therefore contrive the following way to make that tryal. To the plate of the Pneumatick Engine, I apply'd the Receiver V V, open at both ends (ſee *Fig.* 13th.) and the uppermoſt Aperture was exactly ſhut up with a Cover, having a hole in the middle, through which paſſed the ſhank of a Funnel 2 2, and was carefully ſoddered with ſtrong ſodder; the ſaid ſhank at its upper end had but a ſmall Aperture, ſo that I could exactly ſtop it with a little Cement, till the Air was quite extracted out of the Receiver V V: I did afterwards pour ſome melted Lead into the Funnel 2 2, by which means the Cement which ſtopt the Shank of the Funnel was melted in a Moment, and the Lead was very quickly driven into the *vacuum*, and fill'd up the Mould 3 3, and the outward Air following immediately, drove the Metal into all the ſmall ſtroaks; ſo that the Medal which was caſt in this manner prov'd much ſharper than another Medal which I caſt in the open Air: Nevertheleſs I muſt confeſs, that ſince that time I have ſeen ſome things caſt by Founders, without the help of the Pneumatick Engine, which were better than what I had done *in vacuo*, and I make no doubt, but the Nature of the Sand, and ſome Liquor to imbibe it withal, that may have ſome congruity with

with the Metal, are things more eſſential in caſting than the extraction of the Air: Nevertheleſs it is likely that uniting all together, ſomething might be made that would exceed all that was ever done in this kind. And I have thought it would not be amiſs to inſert here this way of caſting *in vacuo*, that thoſe who have the other Secrets may ſee whether by means of this they may improve them: I will therefore adviſe them that it will be good to make the Receiver V V of Metal, for fear a Glaſs one ſhuold break by the heat: The Funnel 2 2 may alſo be made of hamered Iron, by which means Copper, Braſs, and other Metals harder than Lead may be caſt *in vacuo*: And if there be any danger that the Metal falling with great violence would ſpoil the Sand in the Mould, one may put a ſmall Iron Veſſel under the ſhank of the Funnel to break the fall, and this Veſſel may be ſet in ſuch a manner that the melted Metal will from thence run gently into the Mould.

I believe here is Experiments enough not to ſay any thing of thoſe which have been already publiſhed by ſeveral Illuſtrious men, as Mr. *Boill*, Mr. *Hugens*, Mr. *Guericke*, &c. nor of thoſe that will be ſeen in the *3d. Sec.* and theſe would be ſufficient to ſhew the uſefulneſs of theſe Engines, and to engage many people to make uſe of them: But people are wont to imagine great difficulty in ſuch things as they do not know; they believe theſe kind of Engines are good for ſmall Experiments; but can never be applyed to quantities great enough to ſay that they are of real uſe. To that I can anſwer that this is not ſo great a difficulty as we would make it: For there may be found in the Shops here, great wide mouthed Glaſſes pretty cheap which may hold 80. or 100 pounds of Water: So that one may in ſuch a Glaſs ſhut up 60. or 70. pounds of diſſolution of Sugar, raw Fruits, Juices of Plants, or ſuch other things as one may have a mind to: There will remain in that Glaſs a ſpace full of Air able to contain about 20 pounds of Water: And this Air is to be extracted; but this trouble is not great, for it is eaſy to ſhew

that

that to make a *vacuum* of that extent, there needs no more force than to carry a twenty pounds weight to 33 foot high: At least if we use a Pneumatick Engine with double Pump, such as Mr *Boyle* hath described in the beginning of his Book *Exper. Physicomech. contin. secunda.* It is true that there is besides the friction of the peices, but that can't amount to much, especially when the Engines are well made.

It may be imagined also, that it is a thing very difficult when a Glass is *in vacuo* to shut it without letting any Air in; yet it is a thing very easie to be done. I have performed it after 7 or 8 different Manners, but it will suffice to give here that which I prefer to all others.

Vide Fig. 15. A A is the Glass Vessel which containeth the Juice or other matters to be preserved.

B B. a Cover exactly ground upon the aperture of the Vessel *A A.*

C C. a peice of Lead very weak tyed to the Vessel *A A.* and holding up the cover B B. that the Vessel *A A* remaining open, the Air may get out of it freely,

D D. a great Vessel full of Water wherein the Glass *A A* is put.

E E. an other Vessel having its aperture downwards, ground exactly, and applyed to the aperture of the Vessel D D.

F F F. a Pipe inserted in a small aperture at the top of the Vessel E E, and having communication with the Air Pump, which can exhaust the Air that way.

G G. a Brass wire passing through a hole in the Pipe F F, that it may press the cover B B against the aperture of the Vessel *A A* when it is exhausted of Air: This Pipe and this Brass wire ought to be so well tied with Bladders or Eele-skin, that no Air may get into the Vessel E E.

H H. is a cock through which Water may be let in into the Vessel D D, when it is thought fit.

I I. a Pale or Bucket full of Water, in which the Vessel D D. is to be put.

Things being thus diſpoſed, the Pneumatick Engine is to be played to extract the Air through the Pipe F F F: the Veſſels being well emptied, the Braſs Wire G G muſt be thruſt downwards, to apply well the cover B B upon the Veſſell A A: The peice of Lead C C bends eaſily, when it is preſt in that manner: Then the Cock H H muſt be opened, and the water getting in that way will immediately overflow the Veſſel A A: So that being all under Water, there will be no fear of the Air getting in. The Veſſel E E muſt afterwards be ſeparated from the Veſſel D D, and the Glaſs A A taken out; and before the cover be dry, the joint muſt be moiſten'd with Molaſſos round about; and by that means, the Matters ſhut in the ſaid Veſſel may be kept *in vacuo*, as long a time and heated as many times as one pleaſes. As ſoon as a Receiver is exhauſted in that manner, another may be put in its place, and be exhauſted alſo immediately as the firſt. So that in leſs than hours time, many great Veſſels may be emptyed and ſecured againſt the external Air: So that I make no doubt, but by this means great quantity of juices, Fruits and other Matters might be kept for Seaſons, and Tranſported to Places where moſt acceptable.

There is yet another difficulty that hath been objected, eſpecially againſt the Digeſter; they ſay it is true, that an ingenious Man may very well make uſe of it, but Cooks, or Sea Men will never have that Skill: To ſee how weak this Objection is, one needs but conſider the plainneſs of the Digeſter, eſpecially as it hath been deſcribed, *Sec.* 1. *Fig.* 6,7,8; Sea Men can find Witt to do thoſe things they know worth the Labour; they can very well keep their Pumps well, and mend the Plugs, and the Valves when they want; ſo that there is no doubt but they will alſo govern the Digeſter very well, when they will have ſeen how much it will be uſeful to better their Chear, and preſerve them in health. As for the Air Pump it is true, the difficulty to make uſe of it is ſomewhat greater; but yet it is nothing nigh to that which is in the Trade of a *Gold Smith* or a *Watch-maker*, and yet

yet it is known how many thouſands learn them ordinarily, *Apothicaries* and *Confectioners*, *&c.* which are ſeaven Years at their Apprentiſhip, might eaſily give a Month or two of that, to Enure themſelves to make uſe of the Digeſter, and of the Air Pump: Since there would remain time enough yet to them to learn their Trade. It is a ſad thing to be ſo long in arguing for anſwering Objections, that ſeem to be ſo little worth taking Notice of, and the weakneſs whereof doth at the very firſt ſight clearly appear to any one, but indifferently clear ſighted: But it cannot be helpt, ſince there is nothing but ſome ſuch trifles, that hinder the Publick from reaping great advantages from the Benefits of the *Royal Society*.

SECTION

The *THIRD*,

EXPERIMENTS

Tried in

Mr. SAROTTI'S

Academy

AT

VENICE.

WHen Mr *Sarrotti* Secretary of the Senate of *Venice*, left his Reſidence in *England*, in the Year 1681, and retired to *Venice* with intention to keep (as he did) his Library open for the uſe of the Publick, and to ſet up an Academy of Experiments, in imitation of the *ROYAL SOCIETY* of *London*. He took along with him ſeveral Perſons he judged might be uſeful to him in thoſe great Deſigns, and as I had the honour to be one of them, I have had alſo the advantage, the ſpace of two Years, to profit of what hath been

been done and said in that learned Company, and I have had leave to take Copies of what I have found to suite my *Genius*; and I think it will be serviceable to the Publick, if I give here an Abstract of the the things I have gathered there that have some connexion to the Subject of this Treatise: But that I may contribute as much as my mean Capacity will permit to publish, and make the Generosity of Mr. *Sarrotti* known unto the World, which was undoubtedly much beyond that of *Academus*, whose Memory hath been so well preserved: I shall first give here the Discourse that Mr. *Ambroise Sarrotti* his Son made to the Academy the first day they met: in it will be seen the Motives which made him Act.

DISCOURSE

OF

Mr. *AMBROISE SARROTTI.*

"IT is known Sirs by the Testimony of the Antients, how "much the World hath been perfectionated and "embellished, since the time Men are able to preserve the "Memory of what is done in it: dainty Dishes have succeeded wild Fruits: Stuffs magnificent and commodious "to defend us from the injuries of the Weather, have taken "the place of Leaves of Trees, or Skins of Beasts; and instead "of Denns or Cabins, Men inhabit now sumptuous Palaces "where they can in the sharpest Winters, enjoy peaceably all "the

"the Pleaſures that may be expected, of the moſt Sweet "Seaſons of the Year. I believe alſo, that People do not "queſtion, but for theſe happy changes, we are beholding to "ſome induſtrious Men, who taking notice of the things in "the World, and making an Experimental Enquiry of the "Uſes they might be applyed to, have by little and little "brought into the World all thoſe benefits we do enjoy at pre- "ſent: To follow ſuch Studies, is then an Imployment very "worthy true Gentlemen: Since they are ſo uſeful to Man- "kind; alſo we do ſee that in *England*, in *France*, in *Germany*, "& in many other Countries, there are Societies made up of "the moſt learned Men of this Age, whoſe only Aim is to ad- "vance ſtill farther the knowledg of Nature, and the Com- "moidties of this Life. I thought therefore, it would be a thing "of great Moment, if ſome ſuch thing was undertaken in this "our Famous City: You know Sirs, that *Venice* is renowned "for moſt of the ingenious Arts: *Politiques*, *Muſick*, *Limning*, "*Carving*, and many others, witneſs it: there is Reaſon to "believe then, that the ſtudy of Nature will be likewiſe ve- "ry well advanced, if we do once ſet earneſtly upon the "cultivating of it: For that intent I have furniſht my ſelf "both in *England* and in *France*, with moſt of the Engines "theſe Gentlemen make uſe of; and I hope with the help of "your Advices, and the penetration of your Witts, we ſhall "be able to make appear to the World, that we have "as well as other Nations, been Generous and Zealous for the "Publick Felicity.

This Diſcourſe being ended, there were tryed in the Company ſeveral Experiments, to prove that thoſe ſurpri- ſing Effects, which were formerly attributed to the *Metus Vacui*, do meerly depend upon the preſſure of the Air; and as this Doctrine was yet pretty new in that Country, as it found ſome that would ſtand for it; it found alſo many that would ſpeak againſt it: So that many Mee- tings were ſpent about it: But tho' moſt of the Experiments were done with the Air Pump, I ſhall not relate them here, becauſe

becauſe they have almoſt all been ſeen long ſince, in the Illuſtrious Mr. *Boyl*'s Works. I ſhall only relate one here, becauſe I do not think it to have been ſeen any where elſe, and that it ſeems to me a good argument to prove, that thoſe Motions which are uſually aſcribed to attraction are meerly produced by impulſion.

EXPERIMENT. 1*ſt*.

To the Plate A A (*vid. Fig.* 17.) of the Air Pump was applyed the aperture of the Receiver B B, which had a ſmall one at the top: The ſmall Pipe C C was put through that ſmall aperture, and it was well ſtopt with Cement: The Receiver B B had been above half filled with Water; and the pipe D D open at both ends, had been thruſt in the hole of the Plate A A, to hinder the Water from falling out of the Receiver, whilſt the Air that was above it was exhauſted: the top of the little Pipe C C was ſtopt cloſe, and the Pump was plaid. when the higheſt part of the Receiver B B was well exhauſted; the top of the ſmall Pipe C C, whoſe nether end was a little immerſed in the Water inclos'd in the Receiver, was open'd, and immediately the Air entring impetuouſly through the Pipe went a conſiderable way into the Water beyond the nether end of the ſaid pipe: it was concluded from thence that it was not the *vacuum* which was at the top of the receiver that drew the Air in that manner: For how could that Air being eſcaped from the little Pipe C C have gon farther under Water, a contrary way to that towards which the *vacuum* drew it: The *vacuum* ſhould certainly have drawn it upwards as ſoon as the Pipe C C had non hindred it: it was then very plain that this motion was given to the Air by an impulſion from without, which gave it a ſufficient ſtrength to ſink ſo conſiderably under Water. The Famous Mr. *Montanary* had

firſt

first proposed to make this Experiment with a great Tube of Glass five or six foot long which was filled with Water, and whilst the Water fell by the nether end, the Air came in by a small Pipe fitted to the top, this did pretty well; but those of the contrary opinion had still something to say: So that Mr. *Ambroise Sarrotti* Judged needful to make the Experiment with the Pneumatick Engine, as I have described it, and I saw no body had any thing to say against it.

EXPERIMENT 24.

After the company had judged this Matter was cleared enough, it was endeavoured to discover something of the Nature of Metals, by trying the quantity of Air that enters into their compounds: It was pursued after this manner. The weight of 30 grains of Iron filings was put in a Receiver, that can contain 5 pounds, or 60 ounces of Water; a Glass in which was about an ounce of *Aqua-fortis* was also put into it: And when the *recipient* was very well exhausted of Air, the Iron filings were made to fall into the *Aqua-fortis*, and immediately a great ebullition was seen. The Gage that had been put in the same Receiver to know the quantity of Air that would be produced, was observed at the same time to sink as much as if the third part of the Air it ordinarily contains had got in. The *apparatus* remained in the same condition a good while, and so it was certain no Air did enter from without: So that it was concluded that all the Air which had caused the Mercury to sink in the gage, had been produced by the dissolution of the Iron. Upon this Experiment the following calculation was made. Since 30 grains of Iron haue by their dissolution produced as much Air as is needful, to fill up the third part of a Receiver that contains 60 ounces of Water, whose third part is 20 ounces: The Air that have been produced must weigh

weigh the 800*th* part of 20 ounces: For it hath been tryed that a volume of Air weighs about 800 times leſs than an equal volume of Water. Now the 800*th* part of 20 ounces is 12 grains: Then 30 grains of Iron have produced by their diſſolution about 12 grains of Air which is two 5*ths.* of their weight: It was then concluded from thence, that two 5*ths.* of Air enter into the compoſition of Iron. But tho' this concluſion appeared good to ſeveral of the Company, there were alſo others who ſaid it was not ſure: Becauſe the Air which had been produced might have come out from the *Aqua-fortis*, as well as from the Iron: It was reſolved then that other Experiments ſhould be tryed, to endeavour to clear up that matter, and it was begun by the following Experiment.

EXPERIMENT 3d.

Aqua-fortis that had already diſſolved as much Iron as it was able to do, for putting filings of Iron into it it did not work at all, was put under a Receiver and the Air being exhauſted 30 grains of Oyſter ſhels in Pouder were made to fall into the ſaid *Aqua-fortis*, a great ebullition appeared immediately, but this produced leſs Air by half than the 30 grains of filings had done (*Experiment* 2*d.*) The Opinions were differing upon this Experiment: And ſome believed that the Air which is produced in this manner came in part from the *Aqua fortis*: So that the *Aqua-fortis* that had loſt almoſt all its Air in diſſolving the Iron, could not have produced ſo much as that which had not work't at all; the others believed this difference came from the different Nature of the Body's which were diſſolved, without any need of believing any Air came out of the *Aqua-fortis*: So that in the Opinion of theſe Gentlemen Oyſter-ſhels ſhould contain but half the Air Iron doth.

EXPERIMNT 4*th.*

For the further clearing of this matter, *Aqua-fortis* that had diſſoived nothing yet was ſhut up in a Glaſs *in vacuo*, it was put in a diſh of warm Water upon the Fire, ſo that the *Aqua-fortis in vacuo* boiled immediately, and by means of the gage which was ſhut up in the ſame Glaſs, it was obſerved no Air at all was produced: It was continued for ſome time upon the Fire boiling, but it appeared not that any Air was produced by the *Aqua-fortis*: So that thoſe who held the Opinion that the Air is produced only by the Body which is diſſolved, believed the caſe to be plain for them, Since no Air was produced by the *Aqua-fortis* boiling alone; But the others ſaid that the Ebullition which is cauſed by the Fire is differing from that which comes from the diſſolution of the Iron: Becauſe it is likely that at the ſame time the particles of *Aqua-fortis* act upon the Iron, the Iron likewiſe acts upon them: So that being broken, the particles of Air that are in their compoſures may get away. Theſe Experiments then having not appeared ſatisfactory enough, it was reſolved to make that which followeth.

EXPERIMENT 5*th.*

Aqua-fortis that had diſſolved nothing yet, was taken and the ſame Experiment was made with it upon Oyſter-ſhells as had been made Experiment the 3*d.* differing only in this, that in that the *Aqua-fortis* was ſatiated with Iron, and in this it was pure: Both ways produced equal quantity of Air, which pleaſed extreamly to thoſe who believed that the Air that is ſo produced comes out from the Body diſſolved

ved and not from the *Menſtruum*; they ſaid it muſt be ſo, ſince whether the *Menſtruum* hath given up all his Air, or hath given none at all, the ſame quantity of Air is ſtill produced: But thoſe of the contrary opinion were not without anſwer, ſo that it was agreed, that the prooffs we had brought to that time had need ſome others ſhould be thought of to confirm them.

The Experiments about Liquids purged of Air, have alſo much exerciſed the Wits of that Learned Company, and they have made upon them ſeveral new ones by the help of our Pump: By reaſon of the facility of exhauſting exactly the Receivers, and preſerving them exhauſted ſo long as one will. The Honourable Mr. *Boyle* is gon farther in that Experiment than any body, making Mercury remain ſupended in Glaſs Pipes to the heighth of 70 Inches; but the firſt diſcovery of this ſurpriſing *Phenomenon* is owed to *Monſieur Huygaens*, who gave the particulars of it in the *Journal des ſcavants du 25 Juillet* 1672: I gave alſo ſome account of it in the little Tract called *Nouvelles Experiences du Vuide*, Printed at *Paris* in 1674: But notwithſtanding I'le give here again a deſcription of that Experiment to favour thoſe that have not ſeen it, and could not without it underſtand eaſily the other things, which the Academy did to endeavour to Illuſtrate that matter: here is then an abſtract of what was done at Mr. *Sarrotti's*.

EXPERIMENT. *6th.*

Commune Water with a Bolthead or Mattras had been kept the ſpace of 2 days *in vacuo*: So that all the particles of Air who lurk in the pores of the Water, or ſtick to the ſides of the Mattras had had time to extricate themſelves out: The Mattras was fill'd up exactly with that Water thus purged of Air, and its aperture was inverted in ſome

of the ſame Water; they were put in that manner under a Receiver out of which the Air was exhauſted: and it was ſeen, when the Air had been exhauſted as exactly as can be, the Water remained ſuſpended to the top of the Mattras, tho' there remained no Air to ſuſtain it at that heighth. The Pump then was ſhaken, and it happened that a ſmall bubble appeared at the lower end of the neck of the Mattras, it went up along the neck, and being come to the level of the Water in which the Mattras was inverted, this bubble dilated it ſelf and went up to the top of the Water, then the Water fell immediately from the Mattras. The Air being let in again into the Receiver, the Water remounted up into the Mattras and filled it up again, except a ſmall bubble of Air of a pins head's bigneſs. It was tried afterwards if in leaving that bubble of Air in the Mattras, and exhauſting the Receiver again, the Water could remain ſuſpended as before; but it was found it could not, and the Water deſcended by little and little, as the Receiver was exhauſted.

As this Experiment appeared much more new and ſurpriſing than any of thoſe that had been made at firſt for to prove the gravity of the Air, it gave alſo occaſion to many diſputes: Some attributing the Cauſe of it to the ſhape of the Mattras, where the particles of Water upheld one another; being diſpoſed like a Vault; others ſaid that the particles of Water are ſtif, and that being freed from the particles of Air who may keep them aſunder, they do unite and ſettle like Arches who uphold one another till ſome new bubble of Air be form'd which ſpoils their order.

To Confute theſe Opinions Mr. *Ambroiſe Sarrotti* made privately an Experiment, who afforded a new *Phænomenum*, that was not expected,

EXPERIMENT 7th.

This Experiment was made with Mercury which had been purged of Air, in the ſame manner the Water was in the *6th.* Experiment; a Glaſs Pipe ſealed hermetically at one end was filled with that Mercury, and then its open end inverted into Mercury likewiſe purged of Air. This together with another Glaſs Pipe filled with Water purged of Air, and likwiſe inverted into ſome of the ſame Water, was put under a Receiver which being exactly exhauſted, both Liquors remained ſuſpended to the top of their Pipes, tho' no Air remained in the Receiver to uphold them: But when the Pump was ſhaken to make them fall, the Water fell; but the Mercury remained ſtill ſuſpended, tho' the Pump was ſhaken much longer then needfull to cauſe the Water purged of Air to fall down: The company woundred to ſee that Mercury which is ſo heavy a Liquor could uphold it ſelf ſo well; and that on the contrary Water who is ſo light compared to it, ſhould fall ſo eaſily: However becauſe this *Phænomenum* did ſucceed in full Aſſembly as Mr. *Ambroiſe Sarrotti* had foretold it would, hauing already Experimented it in private, it was judged it had not happened by chance, but that it did depend from ſome Natural Cauſe which deſerved to be ſerch't out: It was reſolved then to make upon that Subject ſome other Experiments, and to ſee if a Glaſs Pipe being filled with *Water* and Mercury together, the Liquors would fall as eaſily as if there was nothing but Water; or if the Liquors ſhould remain ſuſpended as firmly as if there was nothing but Mercury.

EX-

EXPERIMENT 8th.

Mercury and Water were put together *in vacuo* to be purged of *Air*, and a Glaſs Pipe ten inches long was fill'd with them: So that every one of theſe Liquors occupyed about 5 inches in the Pipe, the open end was inverted in Mercury purged of *Air*, and then the Receiver under which they were put being exhauſted, theſe Liquors remained ſuſpended together tho' there was no *Air* left to uphold them. Then the Pump being ſhaken they fell very quickly; whereas Mercury alone doth not fall but after a long time and with much difficulty: The liquors alſo fell tho' no bubble of *Air* was ſeen to be form'd; whereas when the Experiment was made with Water alone a ſmall bubble of *Air* was always ſeen to be form'd before it did fall: And this Experiment as well as the 7th. ſucceeded in publick in the ſame manner it had done in private.

EXPERIMENT 9th.

It was reſolved then to illuſtrate ſome what more theſe abſtruſe matters to try the thing again with altering ſome of the Circumſtances, and one day that my Lord Duke of *Northumberland* with ſome Gentlemen of his attendance honoured the Company with his preſence, the ſame Experiment was made with three Glaſs Pipes of which two were ſtraight, and the third had his open end reflected upwards: They were all three fill'd up with purged Water as exactly as could be, except that a ſmall bubble of *Air* had been left purpoſely in one of the ſtraight Pipes. Their open ends were immerſed in purged Water, and they were all together

ther covered over with a glaſs Receiver, as is uſual; the water ſubſided by Degrees in the Pipe where the ſmall Bubble of Air had been left, according as the Receiver was exhauſted; but the Water remained ſuſpended in both the others: Then the Pump was ſhaken, and the Water fell ſpeedily from the ſtraight Pipe; but it was not poſſible to make it fall from the Pipe whoſe end was reflected upwards, tho' the Pump was ſhaken ſtrongly for a conſiderable time. All this ſucceeded alſo, as Mr. *Ambroiſe Sarrotti* had foretold it would, having already tried it in private; ſo that one may rely upon that Experiment, as well teſtified.

That ſame day ſome other Experiments were alſo tryed, and tho' they were not upon the ſame Subject, I'll relate them here; becauſe many may the more rely upon them, knowing they have been done before witneſs's of that worth and Quality, who are now in *England*.

EXPERIMENT. 10*th*.

A ſmall Bell was ſhut up in a *vacuum*, in ſuch a manner, that the ſhaking of the Pump could make it ring; it was at firſt ſet upon a hard Body, and it was perceived, that the Sound was much leſs heard when the Receiver was exhauſted, than when it was not; but yet it was heard: Afterwards the Bell was put upon Cotton, and then when the Receiver was exhauſted it was extreamly difficult to hear any noiſe, though the Ear was placed very nigh; whereas when the Receiver was not exhauſted, the Bell could be heard from the end of the Chamber very eaſily.

Afterwards was made the Experiment of putting the Noſe of a Flute upon the hole of the Pump, through which the Air goes out from the Receiver; and being covered over with a Receiver and part of the Air exhauſted out: When the Air was let into the Receiver again (which muſt be-

be through the Flute) the Flute ſounded loud enough: But when the Receiver was exhauſted more exactly, and that Air ſo rarified was let into the Receiver again, no ſound was made by the Flute.

Afterwards an Alarum Watch was put under a Receiver and when it was not exhauſted, the Alarum was very well heard; but when about three parts of the Air were exhauſted, the ſound was found to be very much diminiſhed, and then the Air being let in again, the ſound was heard as loud as before.

All theſe Experiments proved the Air to be the *Medium*, by means of which the ſounds are communicated, and it was perceiv'd how neceſſary it is to have good inſtruments for Experiments: For the *Florentine* Academy, who have ſhewed ſo much induſtry and capacity, had beſtow'd a great deal of time and expence in making Experiments upon Sounds, and yet for want of good Inſtruments, all the conſequences they could draw from their Labours were contrary to Truth, as may be ſeen in their Book of Experiments.

EXPERIMENT. 11*th.*

Another Experiment that hath been made upon purg'd Water, appeared at firſt difficult to be executed; yet it ſucceeded pretty well as followeth——A ſtraight glaſs Pipe was filled with purg'd Water, and in the ſaid Pipe was a Braſs Wire, who upheld a piece of Lead: This Braſs Wire had its end out of the Pipe turn'd up, ſo that it could be graſpt by the Wire that paſſeth through the top of the Receiver, repreſented *Fig.* 4*th.* ſo that the piece of Lead could be moved up and down in the purged Water that was in the Pipe: When the Receiver was exactly exhauſted the piece of Lead was at ſeveral times moved pretty quick, and yet

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the ſaid Water remained ſtill ſuſpended: but afterwards by ſhaking the Pump, a ſmall Bubble was form'd and the Water fell as it is wont: It was judged then, that the ſuſpenſion of purged Water came not from the Order of its Particles like Arches or Vaults: Since the piece of Lead who diſturbed that Order did not make it fall.

Theſe are near all the *Experiments* I have ſeen upon that Matter at Venice, they are not yet ſufficient to clear it up quite; but they may give a great deal of light and help with thoughts to contrive others, by which we may diſcover the true Cauſe of theſe ſurpriſing *Phænomena*.

EXPERIMENT 12*th*.

The *Air* Pump hath alſo been of uſe to the Academy, in making many Experiments upon Fire; but I ſhall mention but one here, becauſe none but that had a ſucceſs abſolutely new. A ſmall Piſtol ready cock't was put under a Receiver out of which the Air was exhauſted as exactly as poſſibly we could: Then by means of the Braſs Wire that paſſeth through the top of the Recipient (*vid. Fig.* 14.) the Triguer was let fall: but although the Cock open'd very well the pan, the powder did not take fire, nor even were there any ſparks of Fire to be ſeen, tho' the Piſtol was very good, as was found after it had been taken out from the Receiver, for it gave a great Fire twice together, being let of. This Experiment ſurpriſed ſo much the more becauſe it was known that the illuſtrious Mr. *Boyle* had writen the contrary, in his firſt Part of *Experimenta Phiſico Mechanica*: So that ſome of the Company deſired to ſee it tryed in an obſcure place; and ſeeing it ſucceeded in the ſame manner, and that no ſpark of Fire appeared: they concluded, that becauſe Engines that require a great exactneſs, can't be brought to perfection, but with much time; it was likely the firſt Mr.

Boyle had made, had not yet attain'd the utmost exactness; but that certainly, if he had had time to try that Experiment with those Engine he hath caused to be made since, he had seen that no spark of fire can be struckt out *in vacuo*.

EXPERIMENT 13th.

The Academy hath also much exercised it self upon the Subject of breathing; and if I would relate all the Experiments which have been tryed upon it, and the Consequences drawn from them, I should exceed the bounds I have resolved to give to this small tract, and it will be better if I leave that Work to some better Pen. I shall relate then here only an Experiment which hath not yet been tryed any where else that I know of, tho' breathing be a Subject that hath been much workt upon since Air Pumps are made use of.

Some did pretend there was a vast difference betwixt the manner of subsisting of a *Fœtus* in the *Uterus*, and of a Chicken in the Egg; the one *said they*, doth breath, and the other doth not breath. To prove this Proposition, an Egg was brought in which was a Chicken ready to be hatcht: they did pretend that being put *in vacuo* the Chicken should dye, and that would prove, that there is a Communication through the pores of the shell betwixt the Air within the Egg, and the external Air. The Experiment was tried, and after the Receiver had been exhausted much more exactly than would be needful to make a Chicken dye; the Air was let in again, and the Egg was found to be still very hot: some of the Company who believed that the exhausting of the Air had heated it, desired it should be tried if the shutting of it up *in vacuo* again would heat it more, it was done; But it was found that the Egg coming out of the *vacuum*, for the second time was rather colder than warmer.

Then

Then the Egg was open'd, and tho' it had been put twice *in vacuo*, the Chicken mov'd and breath'd as ſoon as the laſt Membrane that folded him up had been open'd: So that it was ſeen that the difference betwixt the *Fœtus* in the *Uterus*, and the Chicken in the Egg was not ſo great as was thought.

To give a more particular knowledg of the Academy, and of the manner things were managed there: I hope it will not diſpleaſe the reader if I give him here a *Specimen* of the explications that were given to the *Phænomena* that occurr'd: I ſhall therefore give here a ſhort Diſcourſe of Mr. *Ambroiſe Sarotti*, which I chooſe rather than any of the others: becauſe it is ſhort enough, and it will alſo inform of four curious Obſervations upon Breathing, the Matter we treat of now.

CONJECTURES
Propounded by
Mr. *AMBROISE SARROTTI*,
Upon the *CAUSES*
OF THE
PHENOMENA
Obſerved
In one of the *MEETINGS*.

FOur things were obſerved in the laſt Meeting, which were curious enough: The firſt was, that Mice and Birds dyed ſooner in factitious Air produced from Dow, than *in vacuo*, as Mr. *Boyle* had already obſerved in his *Exp. Phyſic. Continuatio ſecunda.* The 2*d.* was, that Lizards on the contrary looſe their Motion ſooner *in vacuo*, than in factitious

Air. The 3*d.* was, that Flies live longer *in vacuo*, than Birds: and the 4*th.* was, that on the contrary, Birds remain longer in factitious Air than the Flies.

To explain the first of these Observations to wit, that Birds dye sooner in factitious Air, than *in vacuo*: I'le put the Company in mind of an *Hyppotesis*, propounded by the Famous Mr. *Hook*, which I have already made use of in the explaining of several other *Phænomena*.

It is that Air hath in it self the Dissolvent which causes almost all the Corruptions and Generations which happen in this World, and that being taken in by breathing, it mingles it self with the Blood, dissolves it, makes it to ferment, and so causes it to produce the animal Spirits, which move the Muscles.

I'le observe besides, that factitious Air, such as is that which comes from dow, is an Air which entring into the Composition of Corn had dissolv'd the alimentary Juice of that Plant, and so had lost all its dissolving Power: in the same manner as *Aqua Fortis* after it has dissolv'd Oyster Shells: it is not therefore to be wondered at, that that Air hath not been able to produce in the Blood of a Bird, the Dissolution and Fermentation necessary to Life: But I say moreover, that not only that Air hath not been able to help the dissolution and fermentation of the Blood, but that even it hath much hindered it: an Experiment to prove it, is by putting a piece of Brass in strong *Aqua Fortis*, which will dissolve it very readily: But if you pour upon it *Aqua Fortis* which hath already dissolv'd Oyster Shells, not only this last *Aqua Fortis* will not forward the dissolution of the Copper, But on the contrary it will hinder it extreamly: so that it is seen, why artificial Air kills quicker than *vacuum*; because if *vacuum* doth not afford a new dissolving Power, at least it doth not hinder that which is in the Blood from acting; But factitious Air doth the one and the other of these bad effects.

As

As for the Lizard, the Reaſon that may be given why he remain'd alive longer in factitious Air then he had done *in vacuo*, is becauſe theſe kinds of animals have their blood almoſt of the ſame Nature with that of fiſhes, who need but a very ſmall quantity of Air to ferment it: and ſo they may remain a very long while without Breathing: it happened then that when this Lizard would breath he became ſick, which obliged him to abſtain from breathing, and then the Circulation ſending new blood to the heart gave him his health again: But *in vacuo* beſides the want of Air for breathing, the Lizard had alſo this inconveniency that he had not the External preſſure of the Air to withſtand the elaſticity of the Air engaged in his body which had a great force to divide the *continuum*: ſo that theſe two inconveniencies Ioin'd together kill'd him ſooner than one alone could do.

The third obſervation may be explained with ſaying that flies have their bodies very full of great pores; ſo that when they are *in vacuo* the Air engaged in their pores doth very eaſily get away without ſpoiling any of their organs, which is the cauſe they may endure the *vacuum* longer than Birds whoſe parts being more cloſe, the Air which is engaged in them can't get away without cauſing a great diſorder by expanding it ſelf as it doth *in vacuo*: and that it may not be believed that this poroſity of the flies is a bare ſuppoſition without any ground: one may try that in putting a ſmall drop of oyl upon a flie, it will quickly ſink into its pores and the flie will die.

The fourth *Phænomenum* may alſo be attributed to the ſame Cauſe: For Flyes having ſuch a porous Body the factitious Air into which they are thrown may immediately penetrate their body, and this Air being fit to hinder fermentation muſt alſo immediately deprive them of all motion: But Birds who receive this Air only by breathing, requirea longer time before their motion be quite extinct.

Thus far have I given according to my promiſe an abſtract of what was done by the *Academy* with the Air Pump

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the first Year of their meeting; and I have forborn to speak of the other Subiects they handled at the same time; because they have no reference to this Treatise: Nevertheless that I may by the by give some little notice of those labours, I shall impart here the Speech Mr. *Ambroise Sarrotti* made in the beginning of the ensuing Year: There will be seen a short account of what had been already performed.

Mr. *AMBROISE SARROTTI.*

HIS

DISCOURSE

'IT is Just Sirs. that I may give you my thanks for the Ho-'nour you have done me, to meet here all the last Year, 'to seek out the means of bringing the Sciences and Arts to 'a greater perfection: For besides the pleasure that is met 'with, in receiving a Company made up of so many Inge-'nious Gentlemen, and the advantage that may be reaped 'from their conversations; I estime it so great an honour that 'I can't express enough my thankfullness to those from 'whome I received it: But if in that my thanks are Just, I be-'leive on the contrary that the instances I could make to desire 'you to continue would be very useless and injurious to per-'sons who have already given so many marks of their gene-'rous designs. The work we are about is to extend the Em-'pire of man upon other Creatures and to render him able to 'make of them every day more and more new advantages so

that

"that there is no doubt but you will continue such a com-
"mendable Design since you have already advanced to that
"Degree, that it could scarce be expected, considering the
"little time you have imployed your most curious Wits in
"these *Philosophical* Speculations.

"Notwithstanding the abundance of your other affairs, you
"have already caus'd so many Experiments to be made to
"prove the heavyness, and the Elasticity of the Air, that
"from henceforth there remains little occasion of doubting
"upon that matter which is of Moment, as well because of
"the consequences that may be drawn from it for several
"Engines, as because the Air produceth by that means many
"different Effects upon our Bodies. You have also caused
"many new Experiments to be tryed upon Liquids purged
"of Air, and so you have given a great light to attain to the
"knowledg of that considerable Principle which upholds
"such Liquids to a much greater heighth than the Gravity
"of the Air can do: But because many People doubt of the
"usefulness of these kinds of Speculations: I hope you will
"give me leave to explain it a little to render it sen-
"sible.

"I'll say then in few Words, that when at first it had
"been observ'd, that in sucking Water with Pipes it ascend-
"ed, contrary to the Nature of heavy Bodies, and came into
"the Mouth, it was believed that this proceeded from the
"horrour Nature had of a *vacuum*, and the horrour of a *va-*
"*cuum*, was set as a Principle on which many Effects might
"depend; it was an Error, but nevertheless, this discovery
"afforded many useful Inventions, such as Siphons, Cup-
"ping Glasses, sucking Pumps, *&c.* But as Truth alone hath
"the advantage to maintain it self for ever; the Error we
"speak of, did meet with several disappointments, and when
"in imitation of ordinary Syphons, People have endeavou-
"red to make use of long Pipes, to carry Water over high
"Hills; or when they would by means of the Pump suck up
"Water above 32 foot high, it came to pass, that both La-

"bour

"bour and Expences were quite loſt, and the Principle which "was lookt upon, as very certain did prove defective; "People have therefore been obliged to look for another, "and *Torricelli* having had a ſuſpition that this might proceed "from the Gravity of the Air; there hath been made ſince "that time many Experiments to evince that Truth, which "you have confirm'd by ſeveral of yonr own Invention, and "you have given the Ways how to calculate, what Effects "are to be expected from the Gravity and *Elaſticity* of the "Air; ſo that we are no more in danger to looſe our time "and labour, by expecting from them greater Effects, than "Nature will permit: But alſo in thoſe things that may be "per formed, it may be known exactly enough what big-"neſs, and what ſtrength is requiſite for any *Engine*, and "ſo People may avoid making ſuperfluous Expences; where-"as, thoſe that want Knowledg, are apt to make things "a great deal bigger and ſtronger than need requireth.

"This ſame Diſcovery of the Gravity of the *Air*, hath alſo "given occaſion to find out new Inſtruments, as the Baro-"meter, by means whereof it is known when the Gravity "of the Air doth encreaſe or diminiſh, and ſo the changes of "Weather may be foretold; the Engine for exhauſting of "the Air, by means whereof we have been able to try ſo "many Experiments: The *Mercurial* Gage by means where-"of we may attain to the knowledg of the quantity of *Air*, "that enters into the compoſition of *Bodies*, and many others "which I ſhall not mention here, for fear of being tedious: "I ſhall therefore only beſeech you to conſider, that if the "Principle of the *Metus vacui*, or rather of the Gravity of the "Air, hath by its Diſcovery, given occaſion to ſo many uſe-"ful Inventions, tho' all its ſtrength is no more but to coun-"terpoiſe Mercury to the heighth of 27 Inches; There is "reaſon to hope a great deal more, if we can once come to "a perfect knowledg of that other Principle, from which do "depend the Experiments about Liquids purg'd of Air:

Since

"Since the Illuſtrious Mr. *Boyle* hath already tryed it to be "able to counterpoiſe *Mercury* to the heighth of ſeaventy "Inches.

"It would be too long if I did ſpeak ſo at large, about what "you have done to diſcover, what incredible quantity of Air "makes part of the Metalline Bodies, and of the Fruits: about "the Vegetation of Plants: about the Aſſention of Liquors "into capillary Tubes: about the Nature of ſounds, and about "that of Fire: The Experiments you have tryed upon ſeve-"ral Animals *in vacuo*, and in ſeveral ſorts of Air, either "factitious or compreſt: all your enquiries about the uſe of "Breathing: and the Inſtruments that have been produced, "ſuch as delightful Fountains, Lamps of a new Invention, "Engines to draw a wholeſome food and eaſy to be Digeſted, "from ſuch Materials as were reckoned to be quite uſeleſs, by "reaſon of their hardneſs; others wherewith one may go a-"mongſt Plaguy People without any danger of being infect-"ed: Beſides all thoſe that have been neceſſary to try the "Experiments. I ſhall not I ſay enlarge upon all theſe "things, and I think I have ſaid enough to conclude, that if "ſince the beginning of the World, all Mankind united toge-"ther, could have done every Year, as much as you alone have "done this Year laſt paſt, they would live now as happy in "this World, as in a terreſtial Paradiſe: I will therefore once "more ſay, that I would think it an Injury to you, if I did "in the leaſt queſtion your Intentions to follow theſe Stu-"dies: Since beſides the diverſion you take by exerciſing "your Wits, you do alſo find the ſatisfaction of giving great "Proofs of your Capacity, and the glory of doing good, "which are the chief and moſt real Pleaſures of great "Souls.

This Diſcourſe being ended, they begun again to ſpeak upon the preceding Experiments, and to propound new ones to be tryed: and one of the firſt matters they fell upon this ſecond Year, was the Diſſolutions, wrought by corroſive

Menſtrua, in order to know whether the Air hath any ſhare in that Effect, and ſo to find the ways how to make them quick or ſlow as Occaſion requires.

EXPERIMENT. 14*th.*

There were therefore taken two equal pieces of Iron, and they were put into two ſeveral Glaſſes, with two equal quantities of *Aqua Fortis*: and having ſhut up one of them *in vacuo*, and left the other in the open Air; they remained ſo as long as is neceſſary to finiſh theſe kinds of Solutions: then the two pieces of Iron were taken away at the ſame time, and being weighed, that which had been *in vacuo* was found to have loſt not above a grain of its weight; and the other had loſt almoſt ſixteen grains: This Experiment having given occaſion to believe, that common Air hath a great influence upon Diſſolutions, and being to confirm the *Hypotheſis* propounded in *England* by the Famous Mr. *Hook*, and printed in his *Micrography*, concerning the propriety that Air hath to diſſolve almoſt all ſorts of Bodies, and this *Hypotheſis* having already been of uſe to explain many Experiments that had been made; it was reſolved to make a farther inquiry into it, and the Experiment that follows was tryed.

EXPERIMENT. 15*th.*

Three Glaſſes were taken into which three equal quantities of *Aqua-fortis* were put, and having ſhut up one of theſe Glaſſes *in vacuo*, another in compreſt Air, and left the third in open Air: Into every one of them was immerſed at the ſame time a piece of Iron all of equal weight and figure. The *Aqua-fortis in vacuo* boiled much more and ſoon

er

er than the others: That in compreſſed Air begun laſt of all to boile; but afterwards it boiled ſtronger than that in the open Air, but that great boiling ceaſed quickly; and the ſcum, the Glaſs was filled with, remained almoſt without motion; whereas that in the open Air continued boiling a pace: The Glaſſes were taken out from the *vacuum* and from the compres't Air, and the three pieces of Iron being taken out at the ſame time they were weighed: That which had been *in vacuo* tho' it had made the greateſt ebullition was the leaſt diſſolved; that in compreſſt Air was more; and that in the open Air was moſt of all: So that it was ſeen that the diſſolution had been made ſtronger in the open Air than either *in vacuo* or compres't Air: Thoſe that would not that the Air hath a diſſolving power ſaid immediately this Experiment confirm'd their Opinion: Since compreſs't Air did leſs than ordinary open Air; but they were anſwer'd it was becauſe the Glaſs in the open Air had received new Air continually; whereas the compreſs't Air had been ſoon deprived of its diſſolving power, tho' for ſome time it had acted more ſtrongly than ordinary Air. It was ſaid beſides, that motion being more free in open Air, it might have contributed to make the diſſolution in it greater than in compreſs't Air: And to prove the efficacy of motion, the following Experiment was tryed.

EXPERIMENT 16th.

Two equal peices of Iron were put into two equal quantities of *Aqua-fortis*, and they were left in the open Air until the *Aqua-fortis* began to boil conſiderably in both Glaſſes, then one of them was thut up *in vacuo*, and the other was kept in the open Air: The *vacuum* did not hinder the diſſolution as it had done Expeeiment the 14th, but on the contrary the *Aqua-fortis in vacuo* boiled ſtill much ſtronger than

the other, and when the motion was much diminished in both Glasses both pieces of Iron were taken out at the same time, and being weighed, that which had been shut up *in vacuo* was found to have lost a little more of its weight than the other; but very little to what was expected considering the great boiling it had suffered: So that it was seen that the motion can sometimes supply the want of Air: This efficacy of motion was also proved by the two following Experiments.

EXPERIMENT 17th.

Equal quantities of *Aqua-fortis* and Spirit of Wine were mingled together, two equal quantities of that compound were put into two Glasses, and two equal pieces of Iron were put at the same time into them: Immediately after, one of the Glasses was shut up *in vacuo*, and the other was left in the open Air. The Spirit of Wine did hinder much the dissolving power of the *Aqua-fortis* in the Glass that was in the open Air; but in the Glass that was shut up *in vacuo* the Spirit of Wine caused such a motion that the dissolution was greater than if the *Aqua-fortis* had been alone: For after a considerable space of time, both pieces of Iron were taken out at the same time, and that which had been shut up *in vacuo* was found to have lost more of its weight than the other: From whence it appeared that the dissolution had been there greater than in the open Air.

EXPERIMENT 18th.

The other Experiment that was made to prove the great effect of motion, was by putting two equal pieces of Iron into

into two equal quantities of *Aqua-fortis*, and one being left in the open Air the other was ſhut up *in vacuo*: They were put both at the ſame in an equal heat: That which was *in vacuo* boiled much longer than the other: After ſome time both pieces were taken out together, and being weighed they were found to be equally diſſolved; whereas if there had been no heat the piece of Iron ſhut up *in vacuo* had been diſſolved but a ſixteenth part of what the other had: It was ſeen then that heat cauſing a much greater agitation in liquids ſhut up *in vacuo* than in thoſe left in the open Air, can by that means ſupply the want of the diſſolving power of the Air: It was concluded from thence that the 15*th*. Experiment was not a proof againſt the *Hypotheſis* of the diſolving power of the Air, as ſome had thought at firſt.

EXPERIMENT. 19*th*.

It was reſolved to try yet another way if the efficacy of motion for diſſolutions would be confirm'd by a nother Experiment. Two equal pieces of Braſs were put into two Equal quantities of *aqua-fortis* and when the ebullition began to be great, one of them was ſhut up *in vacuo*; and the other left in the open Air: The Ebull ition appeared much greater in that which was ſhut up *in vacuo*, But nevertheleſs when the pieces of Braſſ were weigh'd that which had been left in the open Air was found to be the moſt diſſolved: for it had loſt four grains of its weight more than that which had been ſhut up *in vacuo*: the ſucceſs of this Experiment being quite contrary to what was Expected: for the thing had hapened therwiſe upon the Iron, *Experiment the* 16*th*, Thoſe than held the *hipotheſis* of the diſſolving power of the Air ſaid it was to be taken notice that the diſſolving power of the Air is more neceſſary in the diſſolution of compacte bodyes; than in that of others: for it had been tried that oyſter ſhells were very well

well dissolved *in vacuo*, tho' they had not begun in the open Air: it had been seen also, that Iron which is more compact than oyster-shells can be dissolved *in vacuo*; but the working must be first strongly begun in the open Air, to the end this agitation may supply the want of the dissolving Power of the Air: so that it is likely, that Brass being a Metal more compact than Iron, it is also more difficult to be dissolved: so that it doth not suffice to put the *Aqua-fortis* in motion, it cannot dissolve the Brass so well, as when it is helpt by the dissolving power of the Air: and so these Gentlemen concluded this Experiment, did rather for them, than against them.

EXPERIMENT 20th.

Another Experiment was yet tryed to prove that the Air helps the Dissolutions: two equal pieces of Iron were put into two equal quantities of *Aqua-fortis*, and when the working began to be great, one of the Bottles in which the Dissolutions were made was shut up exctaly; and the other was left open: the working being finisht, the two pieces of Iron were taken out at the same time, and being weighed, that which had been in the Bottle that had been stopt, was found to be less dissolved, than the other which had been in the Bottle where the Air had free access.

EXPERIMENT. 21.

It was tryed also if the renewing of compress Air could not supply the want of Motion. Two equal pieces of Pewter were put into two equal quantities of *Aqua-fortis*, and one of them being left in the open Aire; the other was shut up in compress Air: and the Engine was ordered in such a man-

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ner, that new Air could be ſtill intruded into it without any danger the Receiver ſhould break: this way is very near the ſame, as that which was given in my firſt Treatiſe of ſoftning Bones, where it may be ſeen, that when the preſſure within the Digeſter was come to a certain Degree great enough, the Valve which was at the top did open, and let out the Air or Vapours, that had made too great a preſſure: I ſhall ſay only, that by this means new Air was thruſt now and then into the Receiver where was the compreſt Air till the diſſolution was judged almoſt ended: Then both pieces of Pewter were taken out at the ſame time, and being weighed, no ſenſible difference was found; whereas when the compreſt Air is not renewed, it hath always happened, that the diſſolution hath been leſs than in the open Air.

It was tryed alſo, if the renewing of compreſt Air ſhould have ſome effect upon the diſſolution of the Iron, and the Experiment was found to ſucceed in the ſame manner as when it had been made upon Pewter, and it was judged, that all theſe Experiments did rather favour, than deſtroy the *Hypotheſis* of the diſſolving Power of the Air.

EXPERIMENT 22*th.*

The Academy hath alſo workt upon the extracting of Tinctures, two equal quantities of red Wood were put into two equal quantities of ordinary Water, and one being left in the open Air; the other was ſhut up *in vacuo*: this took a much ſtronger Tincture than the other, ſo that it was ſeen that *vacuum*, which hinders extreamly moſt of the Diſſolutions, hath a quite contrary effect in the extraction of Tinctures. Afterwards two equal quantities of the ſame red Wood, were put into two equal quantities of a mixture of ordinary Water and Spirit of Wine: and one being left in the

the open Air; the other was ſhut up *in vacuo*: The Mixture ſhut up *in vacuo* took yet more Tincture than the other, but the difference was not ſo great, as in the Experiment that had been made with ordinary water alone.

EXPERIMNT 23*th.*

It was judged this Matter deſerved further enquiry: ſo that the following Experiment was tryed: two equal quantities of powdered Cochenile were put into two equal quantities of Water; and one being ſhut up *in vacuo*; the other was left in the open Air: Both were put to an equal degree of heat: after ſometime the Cochenille in the open Air was found to have given a ſomewhat ſtronger Tincture, than that which was *in vacuo*, and that ſurpriſed, becauſe in the Experiment made upon the red Wood, the contrary had happened, the *vacuum* having much contributed to the quick extraction of the Dye. It was reſolved then to ſee if in trying again this *Experiment* upon Cochenille without heating, the ſucceſs would not be differing; and indeed it was found, that in this manner the Liquor ſhut up *in vacuo* took more Dye, than that which had been left in the open Air: but yet the difference was much leſs than that which had been percieved with the red Wood.

EXPERIMENT. 24*th.*

It was reſolved afterwards to try if the differing Degrees of heat, would not produce differing effects: and as the Degree that had been made uſe of *Exper.* the 23*d.* was very weak; it was reſolved the ſame *Experiment* ſhould be repeated: but that it ſhould be heated with the Flame: and the

the effect of this heat was found very differing from a ſlow heat: for with this quick and ſtrong heat, the Dye was extracted much ſtronger in the Glaſs ſhut up *in vacuo*, than in that which was in the open Air: it was believed this difference of effects, was becauſe the heat made by the Flame had been much ſtronger than the other, ſo that it made the Liquor that was in the exhauſted Glaſs to boil a pace, though the other did not boil, and this great agitation of the Liquor *in vacuo*, had contributed to make an extraction of the Dye ſtronger than the Liquor which was in the open Air had done: however it was ſeen that it was likely this Experiment might become uſeful, becauſe Liquors might be made to boil with much leſs Wood, and ſo draw dyes cheaper than uſually is done.

EXPERIMENT 25th.

This Matter was ſearcht out farther yet in the following manner; powdered Cochenille was put into two Glaſſes with this difference, that there was double more in the one than in the other: equal quantities of Water were put into each of them, and being put to the ſame Degree of heat, the Glaſs where was but the half part of Cochenille was exhauſted of Air: there was left a Tube to make a Communication between that Glaſs and the Pneumatick Engine to draw from time to time the new Air that might be produced by the heat: The Dye was found much ſtronger in the Glaſs *in vacuo*, than in that in the open Air, tho' there was but half the quantity of Cochenille in that *in vacuo*: and even of that, great part did ſtick to the ſides of the Glaſs above the Water: ſo that it could not give its dye; and beſides the Glaſs in the open Air had been heated a much longer time: It was concluded then, that by this means the exhauſting of

the Air, might afford to Dyers a much greater advantage than had been judged by the preceding Experiments.

EXPERIMENT *26th.*

They were alſo deſirous to ſee whether the Air Pump would not be of ſome advantage in extracting Tinctures of Medicaments: for that purpoſe two equal quantities of *Senna* were put into two Glaſſes, with equal quantities of Water, and one of them being left in the open Air; the other was ſhut up *in vacuo*, an equal quantity of *Senna* and Water was put at the ſame time into another Glaſs; and this laſt was put to a moderate heat, ſuch as is uſually imployed for the Infuſion of *Senna*; the Deſign was to ſee whether the *vacuum* could promote the extraction of Tincture of *Senna*, as much as heat doth; and this Experiment would be uſeful, becauſe we can have the *vacuum* conſtantly of the ſame exactneſs; and ſo we might be ſure to have the Infuſions always of the ſame ſtrength; whereas the degrees of heat not being ſo eaſily regulated, there is always ſome danger of making the Infuſions either too weak or too ſtrong: But it was found that the *vacuum* doth not at all effect upon *Senna*, what it had done upon the red Wood, *Exp.* 22*th.* for the Infuſion which was made *in vacuo*, was not found more coloured, than that which had been left in the open Air without heat; tho' the *Senna* that had been ſhut up *in vacuo* had been ſo well penetrated by the Water, that it fell all to the bottom, as ſoon as the Air was let in again into the Glaſs; which did not happen to the *Senna* which had not been ſhut up *in vacuo*; So there was reaſon to think, that *Senna* is of ſuch a volatile Nature, that part of it is diſſipated *in vacuo*; and that was the cauſe why the Liquor did not appear more tinged than that which had been left in the open Air: It was reſolved to make a farther inquiry

quiry into that matter, by infuſing *Senna* in Veſſels exactly ſhut up with Screws, and it was believed the Tincture ſhould be higher in them: But it was not done yet when I departed from *Venice:* The Glaſs that had been put upon the Fire was much more coloured than the others.

EXPERIMNT 27th.

The diſtilling *in vacuo* hath alſo much imployed this learned Company, as being a Matter of Moment: the Inſtrument that was made uſe of, differed but little from thoſe that have been deſcribed in the *2da. Coutin. Exp. Phyſicomech* of Mr. *Boyle*: Neverthelesſs becauſe I think it a little eaſier to be brought to execution, I ſhall give here the Scheme of it, that thoſe who will be willing to work upon this Matter, may chooſe which they ſhall think moſt convenient for their Deſigne.

See *Fig.* 18. *AAAA* are two glaſs Veſſels; one to contain the Matters to be diſtill'd, and the other to receive the diſtilled Vapours.

B B a Tube which makes a communication betwixt the two Veſſels, and their apertures do ſo exactly apply the one to the other, that the external Air can not paſs betwixt.

C C a ſmall Pipe open at both ends, and ſoddered to a hole in the midle of the Tube B B.

D D D another inflexed Pipe one end whereof enters into the Pipe C C, and is tyed to it with an Eel-skin, that no Air may get in; the other end is applyed to the Air Pump, by means of which all the Air is exhauſted from this Inſtrument: it can afterwards be ſeparated without letting in any Air, by means of the Eele-skin which being tyed, but not ſtretcht upon the two Pipes D D C C, it can be ſeparated enough to tye the Eele-skin betwixt them, and afterwards untying it from the Pipe D D, the Inſtrument exhauſted of Air can be carried where one pleaſeth.

The first Experiment that was made with this kind of Instruments, was to put two equal quantities of Water, into two Instruments all alike, and one being exhausted of Air, and the other left with the ordinary Air, they were put to the same degree of heat; and both the Vessels which were to receive the Vapours rested in the same Bason of cold Water: The Vessel exhausted of Air begun very quickly to work, and the Water that was in it boyl'd a pace, tho' that of the *Balneum Mariæ* which heated it was scarce luke-warm: and after a while it was seen, that a spoon-full of Water had been distil'd in that; whereas in the instrument that had been left with the ordinary Air, there was not a drop: so that it was not doubted that the *vacuum* is a means fit to facilitate Distillation.

EXPERIMENT 28*th.*

The preceding Experiment having been judged of Moment, it was thought fit to try it again with Sea Water, all the circumstances above described were then observed, only Sea Water was made use of; the success was like to the other, as for the quickness of the Working in the exhausted Instrument: but it was thought the Water distil'd *in vacuo*, was not so free from its Salt, as that which had been distil'd in the ordinary Air: So that it was thought that could not be useful for the particular Design, to make Sea Water fresh; tho' on the other side, this propriety of altering but little the distil'd Matters be extreamly advantagious in other Occasions.

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EXPERIMENT 29*th.*

Afterwards another Inſtrument was made for Diſtillation much like that which hath been deſcribed *Exper.* 27*th.* but with this difference, *vid. Fig.* 18. that in this the Pipe BB which makes the communication betwixt the two Veſſels, *A A* was 20 Inches high, and was in part made of Glaſs, ſome Wine was put into this Inſtrument, and being exhauſted of Air, the ſide that contained the Wine, was put into a *Balneum Mariæ* deep enough, that the degree of heat might be meaſur'd by the means of a Thermometer fitted to know the ſpecifick Gravity of Liquids, and by this means we might know when the Water of the *Balneum Mariæ* was heated, as much as was neceſſary to make it one hundred & 10*th*, part lighter, than it is in the ordinary ſtate betwixt the cold ready to freeſe, and the great heat of Summer: We kept the Inſtrument more than an hours time in that ſtate, and yet the Vapours could not get up to the top of the Pipe B B which made the communication; and the greateſt heighth to which ſome ſmall drops were perceived, was the heighth of 15 Inches: This Experiment ſeem'd to prove, that Vapours can't get up ſo eaſily to a great heighth *in vacuo*, as in the ordinary Air; for when they rectify Spirit of Wine, even the aqueous Particles get to a much greater heighth, tho' they do not apply a greater degree of heat, than that which was applyed in this occaſion: So that it was believed theſe two Conſequences might be inferr'd

1. When we are to diſtill *in vacuo*, it will be often requiſite to take care that the Vapours may have but a little way to get up, that the Diſtillation may be facilitated; for if we would have the Vapours get up too high, the *vacuum* would hinder inſtead of helping.

2. That

2. That it may be hoped the *vacuum* will be useful for Rectifications; for the aqueous Particles having so little strength to get up *in vacuo*: and the spirituous having much more, there is reason to hope, that without much difficulty, Experience will teach the heighth instruments are to have, and the degree of heat fit to make that Spirits be distill'd quick enough, without fearing the aqueous Particles may get vp with them.

EXPRIMENT. 30*th.*

It was resolved afterwards to try what effect the distilling of Flowers *in vacuo* would have; to that end two equal quantities of Roses were put into two like distilling Instruments; and one of them being exhausted of Air, the other was left with its ordinary Air; and they were put to the same degree of heat; about half a spoon-full was distil'd in the Instrument exhausted of Air: but in the other there was none at all; so that it was judged, that the extracting of the Air would be very useful for the stilling of Flowers; because being distilled speedily in *balneo Mariæ* it will not be necessary to distill them in Sand, or by open Fire which gives an empyreume to the distillation: Besides there will be no fear the most subtile parts be lost, but all the sweet smell will be preserved: and indeed when the instrument was open the Water that had been distilled was found to have more sweet smell than *Rose-water* uses to have: And a very remarkable thing is, that this distilled Water having been kept some dayes, did congeal: which is a thing that doth not happen to ordinary distillations.

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EXPERIMENT 31*th.*

The extraordinary effect of the preceding diſtillation, gave the Curioſity to try what effect the operation would have upon *Orange* peells, two equal quantities of them were then put into two like inſtruments for diſtillation, and being put in the ſame *Balneum Mariæ*, that they might recieve the ſame degree of heat, one of them was exhauſted of Air; and the other was left with its ordinary Air, a quarter of a ſpoonful of Liquor, was found diſtilled in that which was exhauſted of Air; but in the onther there was no appearence at all that any vapours had paſt into the Receiver: So that it was ſeen that in this diſtillation, as well as in thoſe that had been made before, the *vacuum* facilitated much the operation. Afterwards the recipient exhauſted of Air was open, and the Liquor that had been diſtilled in it was found to have a perfect good ſmell, and that ſeveral ſmall drops of Oyl did ſwim upon it, which gave occaſion to think this Experiment might be uſeful, becauſe the Oyle that is generally diſtilled of *Orange* peells hath not a good ſmell.

EXPERIMENT 18*tht*

After the Flowers and the Peels, they would alſo try ſome Fruits, an Experiment then abſolutely like to that which hath been deſcribed for the *Orange* peels, was tryed upon *Plums:* The *Plums* that were *in vacuo* made a greater Ebullition, that is to ſay that a great quatity of bubbles of Air came out from their pores: and the vapours were alſo quickly ſeen to condenſe in the other ſide of the inſtrument, and after a while about half a ſpoonful was ſeen to be diſtill'd in that,

that, tho' not any vapours did appear yet in the other inſtrument which had the ordinary Air: The exhauſted inſtrument was opened, and the Liquor that had been diſtilled in it was found to have the perfect ſmell of the Fruit: So that it was believed this manner of diſtilling would be alſo uſeful for the Fruits, and that it would ſerve to render the Operation more quick, as well as to preſerve all the volatile parts, and all the virtues of Fruits that are to be diſtilled.

A Digreſſion to Explain *the foregoing* Experiments.

I can not proceed any farther without telling ſomething to prevent the wonder which may ariſe from ſo many Experiments and eſpecially from this laſt made upon Fruits, which do all confirm that the *vacuum* is fit to haſten extreamly the diſtillation, and nevertheleſs we have ſeen an effect abſolutely contrary *Sec. 1ſt. Exp.* 43*d.* the which Experiment was alſo made upon Fruits, and yet the Operation in the compreſt Air was without compariſon greater than *in vacuo.* I ſhall endeavour then to explain the cauſes of the one and the other of thoſe effects, that thoſe who cannot convince themſelves of them by Experience may not laugh at them as things that contradict one an other.

Notice is to be taken then that all the diſtillations made at *Venice*, were done by a very ſmall degree of heat; So that none but the moſt volatile parts and little intricated could be put into motion, whereas in the 43*d. Exp.* of the 1*ſt. Sec.* the heat was ſo great that it ſeparated particles much intricated: it remains then to know.

1. Why Particles little intricated are diſtilled quicker *in vacuo.*

2. Why Particles more fixe are on the contrary diſtilled quicker in the compreſt Air.

Upon the firſt of theſe *Phenomena* I ſay that the volatile par-

particles have almoſt no need of any ſtrength to diſunite them, and it ſufficeth that the heat makes the preſſure a little greater in one ſide than in the other, to make them paſs quickly into that ſide where there is leſs preſſure: And if it is ask't why the difference of preſſure cauſed by the heat, cauſeth not this effect in the compreſt Air as well as *in vacuo*: I ſay that *in vacuo* the preſſure cauſed by the heat is the only preſſure that is there, and ſo it doth eaſily make its effect there being nothing to reſiſt it; as a man being alone in a Room could eaſily carry a weight from one ſide to the other: But if beſides the Man I ſpeak of, there was a hundred Men on a ſide of the Room and a hundred on the other, which ſhould thruſt one another; and the Man I ſpoke of at firſt would joyn himſelf to the hundred Men of his ſide to break the *equilibrium*, and to be able to paſs on the other ſide, by cauſing the other party to yeild; it is eaſie to judge that this would be done much ſlowlier than if this Man was alone. The inſtrument with the ordinary Air then having a great preſſure on both ſides, the little addition the heat mak's to one of them can't produce but an effect much ſlower than when its preſſure is alone.

As for the 2*d*. *Phænomenum* to wit why the Particles more fixe and more intricated are diſtilled more abundantly in the compreſt Air, it can be explained by continuing the ſame compariſon, and ſuppoſing that the weight or matter that is to be tranſported, can't be taken away unleſs it be firſt bruiſed and put to pieces, by treading or ſome other motion of the Men which thruſt one an other: for then it is eaſie to judge that the greater number of Men will be the more able to bruiſe and diſunite a greater quantity of that matter, which afterwards will be tranſported in greater quantity than if there was few or no Man to bruiſe it: So that when there is much compreſt Air, which diſunites and diſſolves the ſaid Particles of Fruits, it is no wonder a greater quantity be diſtilled, than when thoſe Particles remain fixed together for want of Air to diſſolve them. It can be concluded from all this that diſtillations that require

a conſiderable heat, are to be made in compreſt Air; but for things that are very volatile and which require a ſmall heat, the *vacuum* will be a great deale better.

EXPERIMENT. 33*th.*

As *Jeſſamine* hath qualities very fit to comfort the heart, as well as to pleaſe to the ſmell, it was judged to be very worth while to make ſome tryal of it: *Jeſſamine* Flowers then were put into an inſtrument fit to diſtill *in vacuo*, and being exhauſted of Air it was put to a moderate heat of the *Balneum-Mariæ*: When about a quarter of a ſpoonful of Liquor was diſtill'd, the Inſtrument was open, but that Liquor was not found to have ſo ſweet a ſmell as it had been hoped, which gave occaſion to think this ſent is of ſuch a volatile ſubſtance that it flyes away in the ſame time that the Glaſs is exhauſted; or elſe that the heat tho' very moderate had yet been too great, and that the beſt would be to uſe no other heat than that of the Sun, to diſtill Flowers as tender as theſe.

EXPERIMENT. 34*th.*

They have alſo tryed what effect *vacuum* would have upon the diſtilling of fermented Liquors: Some Wine was put to be diſtilled into two inſtruments a like with this only difference, that one was with its ordinary Air; and the other was exhauſted of Air: The inſtrument that was exhauſted of Air begun a great deal ſooner than the other to work ſenſibly, and the Liquor that was diſtill'd was found to be very good Brandy: So that it may be expected that by the means of the Air Pump, Brandy may be made quicker and cheaper than it is uſualy done.

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The Air Pump hath alſo been of uſe to make ſeveral Experiments in Anatomy: one of them hath already been ſeen printed in the *Journal des ſcavans* of the *6th.* of *March* 1684. and it hath been ſo well recieved, that the Publiſhers of the *Nouvelles dela Republique des Lettres* in *Hollande*; and of the *Acta Eruditorum* in *Germany* have judged it worth while to tranſcribe it into their Works; I ſhall not therefore deſcribe it here now, but rather make an end to theſe Papers: Becauſe being departed from *Venice* I could not be witneſs of what hath been done ſince in the Academy: Yet we may be ſure that after having paſt over the difficulties that do uſually attend the beginning of any great undertaking, theſe Gentlemen did afterwards proceed even more ſucceſsfully than they did at firſt: But at laſt the Senate having judged it neceſſary to ſend Mr. *Sarrotti* into *England* again with the ſame Charracter that he had ſo well ſuſtained heretofore, he hath been obliged to leave off theſe deſign's, and it is already ſome time ſince the Academy is over. But altho' it hath laſted ſo little it hath nevertheleſs produced conſiderable effects, ſince beſides the good diſcoveries they have made, it hath alſo ſhewn how much may be expected of *Venetian* Witts when they will be willing to apply themſelves to that Study: and there is reaſon to believe that ſince this wiſe Republick hath been able ſo long a time to maintain its Subjects with ſo much Tranquillity in the enjoying of the great Riches they have already acquir'd; it will alſo encourage them to acquire continually new ones by diving into the unexhauſted Treaſures of Nature.

F I N I S.

POSTSCRIPT.

SInce the Printing the 10*th,* Experiment *of the third* Sec. *about a Piſtol producing no* Sparks in vacuo, *the thing hath been tried in the* Royal Society, *and the Piſtol was ſo good that the triguer falling, gave ſo great a light in the dark, that ſome would think that there had been ſome* Gun-powder *kindled in the Pan: But when the Air was exhauſted and the triguer cauſed to fall* in vacuo, *no ſparks were to be ſeen even in the dark. Nevertheleſs my Lord of* Pembrok *ſtood in ſuch a place where he might ſee the firſt Colliſion of the Steel and Flint, and in that place at the time of the Colliſion, he could diſcern ſome faint Light in the Dark; but to be ſurer of it he cauſed ſome Gentlemen to ſtand in the ſame place, and the Experiment being repeated they obſerved the ſame Light: From whence it was concluded that Light may indeed appear meerly by a violent Motion, but that no Sparks of Fire can be produced without the intervention of Air: And it is likely when they ſaw no Light in the Dark at* Venice, *it was meerly becauſe it fell out that no body ſtood in the place from whence it might be obſerued.*

ERRATA.

Page 18. *line* 21. for *to large* read *too large. p.* 28 *l.* 6. for *to ripe* read *too ripe. p.* 36. *l.* 4. for *to it theſe,* read *to it and faſtned in a frame, theſe. p.* 36. *l.* 22. add *vid. fig.* 9. *p.* 38. *l.* 8. *ſec.* 2. 21. read *ſect* 2. *exp.* 21 *p.* 46. *l.* 9. for *fig.* 25. read *fig.* 15. *p.* 47. *l.* 9. for *freely: run* read *freely run p.* 81 *l.* 13 for *fig.* 15. read *fig.* 16.